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SOME THEORETICAL AND SOME PRACTICAL ASPECTS OF PSYCHANALYSIS*

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PHILADELPHIA

Is psychanalysis in part or in whole a form of mysticism or semi-mysticism? The affirmative of this question is strongly resented by most psychanalysts.

I may fail to apply academic logic to my consideration of psychanalysis, neglecting references to secundum quid, tertium quid and ambiguous middle. With regard to the last, both the defenders and the opponents of psychanalysis not infrequently have shown weakness and confusion both fore and aft as well as in the middle. Let us first see what the psychanalysts say of each other.

One of the first to challenge the hypothesis of Freud as savoring of mysticism was Jung.[1] "It must be admitted," says Jung, "that Freud's school makes use of the conception of libido which passes beyond the bounds of its primary definition. Indeed this must produce the impression that one is working with a mystical principle."

Jung, who thus challenged Freud, shows himself at times to be a disciple of the occult. Witness, for example, the manner in which he uses the symbolism of numbers. The use of numbers in symbolism by the psychanalysts naturally recalls those followers of occultism who are known as numerists or vibrationists, of whom it is sufficient to say that to the numbers from one to nine very special significance is given. Number one, for example, has a vibration from the sun; number two from the moon; number three has a larger ray of symbolic significance besides the psychanalysts' directly phallic interpretation; five has the strongest psychic vibration.

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* Read at the Forty-Seventh Annual Meeting of the American Neurological Association, Atlantic City, N. J., June, 1921.

1. Jung, Carl G.: The Theory of Psychoanalysis, Nerv. & Ment. Dis., Monograph Series, No. 19, New York, 1915.

While Jung has seen fit to call Freud a mystic, on the other hand, Ernest Jones,[2] a close follower of Freud, says of Jung's conception of the unconscious that it is founded on pseudophilosophy irreconcilable with science. Jones holds that Jung's views are characterized by a repudiation of determinism. He puts Freud with Huxley and Darwin and, briefly stated, calls Jung a religious mystic.

The faulty, fantastic or mystic character of psychanalysis is emphasized by certain prenatal references in its literature. Ferenczi,[3] for example, mentions one case in which he had partial or temporary success with hypnosis which he believed revealed a so-called mother complex, but later, by the use of psychanalysis, a father complex was also revealed. He says, regarding this case, that there was "at the bottom a symptom of unconscious phantasies connected with infantile ponderings on the menstruation and gravidity time periods with his mother, and amongst others on the idea of his own situation in the womb and at birth."

One might recall the "tunnel-window case" in which a ploughed field comes into view, recorded by Freud[4] and cited by Maeder[5] and others.

The symbolic interpretation, according to Freud, is that of the intra-uterine fetus spying on his parents in the act of coitus.

In the same category with these assumptions of Ferenczi and Freud belongs Clark's[6] libido conception of the origin of epilepsy with its infantile and prenatal references.

I do not know from anything I have read whether Adler[7] has accused other advocates of psychanalysis, or has been accused himself, of mysticism. It looks, however, as if his special theory of organ inferiority and psychic compensation—he being a sexual psychanalyst—has been developed as a sort of defensive reaction against being considered a mystic himself. At first sight his theory is somewhat appealing to those who tend to look for organic causes, but after well presenting his case in a series of most instructive arguments and illustrations, he

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2. Jones, Ernest: Why Is the "Unconscious" Unconscious? Brit. J. Psychol. **9**: 247 (Oct.) 1918.

3. Ferenczi, S.: Contributions to Psychoanalysis, authorized trans., Dr. Ernest Jones, Boston, Richard G. Badger, 1916.

4. Freud, Sigmund: Three Contributions to the Theory of Sex, Nerv. & Ment. Monograph Series, No. 7, New York, 1916.

5. Maeder, A. E.: The Dream Problem, Nerv. & Ment. Dis. Monograph Series, No. 22, 1916.

6. Clark, L. Pierce: Epilepsy a Manifestation of the Imperfectly Developed Libido in Persons of a Psychosexually Inferior Makeup, New York M. J., Feb. 27, 1915; Am. J. Med. Sc., November, 1919.

7. Adler, Alfred: Study of Organ Inferiority and Its Psychical Compensation, Nerv. & Ment. Monograph Series, No. 24, 1917.

with one sweep accedes to the sexual causation and interpretation of various forms of neuroses and insanities. He confirms this interpretation of his position by bringing into the discussion what is so much beloved by psychanalysts, the one legged, one handed, one eyed, dumb and lame hero myths of occult literature in whose deeds of blood and sexuality, as in his patients, he sees psychic compensation for physical inferiority.

In MacCurdy's[8] pamphlet on the "Ethical Aspects of Psychoanalysis," we see here again what appears to be a defense against accusations of the possible mystic or at least illogical position of psychanalysis. MacCurdy says that "considering the trifling degree of correlation as yet established between structural alteration and brain function, these investigators are in the position of men, so convinced that all water comes from the clouds that they will not think of damming the river in a flood."

Is it true that the correlations which have been established during the last fifty years between structural alteration and brain function are so trifling, after all, in the light of what we have learned through anatomy, morphology, physiology and clinicopathology, regarding the cerebral representation of motion, sensation, tonicity and speech, and even of emotion and concrete conceptual and abstract thinking? Are these not worth more than all the purely psychic speculation regarding mental phenomena?

It will be recalled by those who have read the book of Kempf[9] that this psychanalyst may be said to harp on one or two strings throughout his entire volume of nearly 800 pages. The reader is reminded through chapter after chapter of the strivings of segments against the ego. The impression is conveyed that with regard to such questions as emotion, sexuality, and the like, the personality is entirely dominated by the segmental visceral system. The author makes use of the excellent experimental researches of Cannon but in considerable part at least misapplies them. To such an extent is this done that one almost gets the impression that for Kempf the mind is resident in the spinovisceral segments of the nervous system. It is a sort of recurrence, as someone has said, to the so-called abdominal brain idea of personality.

The ego assumed in Kempf's work is a rather mystical being. The author seems to deny anything like a mind or soul or indeed an active controlling brain, and uses a sort of verbal or assumed ego to help him out of many manifest difficulties.

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8. MacCurdy, John T.: Ethical Aspects of Psychoanalysis, The Johns Hopkins Hosp. Bull. **26**: No. 291 (May) 1915.

9. Kempf, Edward J.: Psychopathology, St. Louis, C. V. Mosby Co., 1920.

Kempf, more than he intends, seems to make an autonomic segmental subdivision of our organisms the controlling power. He gives us no clear idea of what he means by consciousness and seems to believe that it is in some way the expression of the entire personality of the individual—whatever that means. He would have us think with our muscles, control with our glands and viscera, and would make almost all psychotic diseases simply a struggle between affective cravings of segments and the hypothetical ego. All this is very fine, but, after all, does it get us anywhere in the final shakedown of theories relating to mind and matter?

Being a freudian, Kempf, of course, lays tremendous stress on the sexual elements in the analysis of cases. All through his book he brings out illustrations wherever he can of some boy or girl or woman or man having struggled with sexual tendencies which have more or less conquered his ego, or with which his ego has been struggling.

One can easily award a certain amount of credit to Kempf for collecting a large number of cases from St. Elizabeth's Hospital and other sources which illustrate the fact that sex or sexual ideas are often prominent in the minds of the insane, but this proves little or nothing in substantiation of his main position. Over and over again it is evident, although he speaks at times of evolution and that sort of thing, that the highly developed or highly involved human brain plays only a secondary part in the pathogenesis of the psychoses or neuroses with which he is dealing. Like most freudians he is suggestionally fascinated with his views of psychology and psychiatry. Sex is glaringly exhibited to him on every psychiatric canvas. In the book there is an extraordinary disregard of all the neuro-anatomic, neurophysiologic and neuropathologic work on the brain which has been done in the last half century.

I cannot refrain from making brief allusion to the pictures with which Kempf illustrates his volume, or rather to the legends by which he has described these illustrations. These legends furnish another example of the manner in which the psychanalyst reads into any subject the ideas which for the time being are dominating his mentality. The manner in which in one of the interpolated pictures a chasm is made to symbolize the vagina, its bordering cliffs the thighs, the foliage in the background, pubertal hair, a shadowy stream for which a boat containing a standing figure in white is heading, the amniotic fluid and all the rest are indications of the way in which the advocates of psychanalysis can be made to misuse what purports to be sexual symbolism. I cannot take the space to refer at any length to the manner in which Kempf and others have seen fit without warrant to read into paintings and statuary and much else their own ideas as to

symbolism. If Michael Angelo or some of the other great worthies in art could be summoned from the astral plane to which they have been ascribed by another mystic cult, they would indeed be amazed at the meanings, never dreamed of by them, which have been given to the details of some of their great works.

Sherrington has clearly shown that an animal whose cord has been completely transected, will definitely respond with emotions like rage and fear although the animal may not exhibit some of the local phenomena of such emotions. I have seen many cases of high cervical lesions, a few complete in one or in all spinal systems, in which the power to respond to emotion both psychically and by facial and ocular expression was retained.

Observations on certain cases of paralysis agitans and pseudo-paralysis agitans are interesting in connection with this question of the abolition of emotional expression with which at least in some instances goes also a reduction in emotivity. I have reported a number of cases in which emotional expression and emotion itself were disturbed as the result of a lesion of the cerebral portion of the cerebro-spino-autonomic nervous system.

Without the concept of the unconscious, recent psychanalysis would have nothing on which to stand. To define or explain consciousness is by no means easy. Even so great a psychologist as William James [10] when lecturing on the stream of consciousness, tells us that he is not prepared to give an answer to the problem on the explanatory level; in other words, he declines to define the term and confines himself to the descriptive or analytic level.

The term "awareness," sometimes used in the definition of consciousness, is not much more explanatory than the word consciousness itself.

Let us turn for a moment to the views of three different writers on psychanalysis. At the joint meeting of three British societies concerned with psychologic questions, a symposium was held, during which was discussed the rather cryptic question "Why Is the 'Unconscious' Unconscious?" Those presenting leading papers in this symposium were Maurice Nicoll, W. H. R. Rivers and Ernest Jones.[11]

Nicoll, a disciple of Jung, largely confines himself to a presentation of the views of this psychanalyst. In much of his paper he concerns himself with what he terms the "collective unconscious" by which he seems to mean the "accumulated ancestral unconscious" of a person,

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10. James, William: Talks to Teachers on Psychology; and to Students on Some of Life's Ideals, New York, Henry Holt & Co., 1901.

11. Nicoll, Maurice; Rivers, W. H. R., and Jones, Ernest: Why Is the "Unconscious" Unconscious? Brit. J. Psychol. **9**:230, 1919.

not simply for a few generations, but the unconscious of our thousands and hundreds of thousands of ancestors during a million years. To him the unconscious is not a personal but an impersonal matter. After all, this is only another and not very satisfactory way of stating the great question of heredity. One may inherit traits, trends, tendencies, capacities or incapacities, but never an isolated memory or thought.

Rivers, who follows Nicoll, disagrees not only with him but also with his own successor, Ernest Jones. The chief tenor of the contribution of Rivers seems to be that what is repressed within the unconscious is chiefly that part of the instinctive which is not useful to the developing personality of the individual. His view in brief is utilitarian. He makes use of the well-known illustration of the larva and butterfly and also of the views of Head and his collaborators, of whom Rivers was one, regarding protopathic and epicritic sensibility.

Ernest Jones assails the point of view of both Nicoll (Jung) and Rivers. He considers neither of them tenable and attacks Jung's idea of the unconscious with a big stick in a way to which I have already referred in discussing mysticism. Jones is a hedonist and accepts the pleasure-pain theory in explaining sexual repression in the "unconscious."

In another article, Ernest Jones [12] discusses what is meant by the unconscious. He first speaks of what he terms the mental theory meaning that of no mentation occurring, as when a patient is unconscious from a knockout blow. This is the sense in which some psychologists use the term. The second theory of the unconscious to which he refers is what he terms the limbo theory, which makes the unconscious a lumber room for all sorts of forgotten or discarded memories. The third form of the unconscious Jones calls the scientific view by which he means the freudian theory. After all, this freudian view of the unconscious is also a limbo hypothesis, the one point being that the sexual psychanalysts only refer matters sexual to their limbo.

In the process of my psychanalytic reading I tallied new terms until they had reached four score or nearly this figure and by no means exhausted the subject. A voluminous terminology is often a sign of weakness rather than of strength in a science or art. A somewhat extensive terminololgy is of course necessary in some of the more exact sciences like botany or physics.

In psychiatry in particular—largely because of the uncertainty of our facts and theories—classification and classifying terms have been sadly misused. The tendency is sometimes shown by men who are fond of going into minutiae, to take the insanities with well established symptoms, lesions or psychic phenomena, and divide and subdivide

12. Jones, Ernest: Papers on Psychoanalysis, New York, William Wood & Co., 1919.

them until perhaps, as in the case of one of our late colleagues, we are presented with a classification which has as many varieties and subvarieties as Heinz's well-known collection of pickles.

In psychanalysis this tendency to division and subdivision, to the invention of new terms or the application of old terms to new uses has been carried to excess. A long list of the terms thus introduced into psychanalysis could readily be made—in fact, has been made. Each new investigator is not happy until he has invented at least a few new terms.

In the first place, we have the already considered necessary terms, consciousness and the unconscious, but various other terms have been added to these, such as preconscious, the foreconscious, the subconscious and the coconscious.

Starting with varieties of consciousness, we are then confronted with libido, fixation, repression, sublimation, regression, compensation and overcompensation, reaction, conversion, constellation and many other terms. The use or misuse of these terms needs continuously to be kept in mind in reading the contributions on psychanalysis; nor is this use consistent with different authorities. In the case of libido, for instance, Freud gave it a meaning which closely associated it with sexual instinct or desire, but Jung tells us it should have a more general energic significance, with which statement the freudians later have more or less coincided. Sexual trauma becomes sexual phantasy or sexual experience.

A few terms have particularly appealed to me as of dubious utility, such as narcissism, constellation and complexes, in all their extraordinary array. We all know the meaning of constellation as applied in astronomy, but its transfer to the domain of psychanalysis is one of the rhetorical methods of exploiting the subject. By constellation seems to be meant a group of related ideas. Why not then be satisfied with calling them a group of ideas, without invading the domain of the astronomer?

The term narcissism, introduced by Havelock Ellis, and speedily adopted by Freud, is another rather pleasant word which derives its meaning through the pretty myth of the youth Narcissus whose admiration of his own image reflected in the pool led to his eventual destruction. Around this word has been made to evolve a series of ideas relating to self-love and especially to libidinous self-love.

The method with which Socrates—at least as represented by Plato—perforated the terminologic bladders of the ancient sophists, letting out their hot air and obnoxious gasses, might equally be used in attacking the terminology of these modern freudian sophists. It could be readily

shown that the large use of terms by the latter is in not a few instances defensive, serving to get them over hard places.[13]

Of all the terms which have been invented and amplified, that of complex is perhaps the most striking. It was, I believe, originally applied by Jung and his immediate followers to extricate themselves from the difficulties which arise in the consideration of ideas in the unconscious supposed to have emotional or instinctive associations. Freud, ever on the alert for new philologic weapons, after a little resistance, adopted the term complex, until now the freudians in common with those of other psychanalytic schools revel in complexes—hidden, anal, oral, castration, and finally the root or nuclear complex which has its exemplification in the Oedipus complex. The Oedipus complex and the Electra complex, especially the former, are the favorites of the psychanalyst.

Some psychanalytic writers, like Jelliffe, for instance, tell us that this Oedipus complex can be used as a sort of psychic measuring unit or foot rule. We must look, according to the Oedipus hypothesis, for some phase of the relations which the child bears to his parent of the same or of the opposite sex, of the son to the mother or father, of the daughter to the father or mother, or of the sister to the brother. Paramount in the hypothesis is the idea that these relations are in some way of a libidinous or incestuous character.

Freud, Rank, Ernest Jones, Jelliffe and others work this story of Oedipus to an extraordinary degree. Given an ancient mythologic story, a tragedy, old or more recent, a novel like Defoe's "Moll Flanders" which seems to have been rather neglected by the psychanalysts, a painting or a piece of sculpture, the psychanalyst is able to read into or out of any or all of these his preconceived hypothesis.

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13. Since the delivery of this address in June, an article has appeared in the December to March number of the Journal of Abnormal Psychology, on "Education and Freudism," by George Humphrey, which I did not see until August of this year. In this article the author ably discusses the subject of terminology more fully than I did in my address, the difference being partly due to the fact that I had to confine my paper within certain time limits. Humphrey shows clearly the manner in which terms have been given an exaggerated importance and just as clearly states what I had in mind and briefly expressed, that the terms used in psychanalysis are often paraded as if they were actual entities instead of more probably helps to the exposition of false psychic hypotheses. Humphrey is particularly clear with regard to the use of such words as conflict, projection, interjection, and the like, so attractive to the exponents of freudianism. These terms, he holds—and I agree with him—are given utterly unjustifiable valuation, and much of what they are supposed to illustrate can be readily explained on the principle of the conditioned reflex.

One of the first of these efforts was that of Ernest Jones to discover the Oedipus complex in the drama of Hamlet, a task in which I think he was not altogether successful, as was pointed out by Lloyd.[14]

Jelliffe [15] tells the story of a patient with various evidences of inherited and acquired imperfections and derelictions to whose complex the psychanalyst could not obtain a clue until at last he learned of a monotonous breakfast in which his patient had indulged for fifteen years, consisting of waffles and maple syrup and sausages, rather small. A further inquiry into the patient's dietary revealed the fact that for years he had lived chiefly on milk, potatoes and meats. The application of the Oedipus complex to this breakfast and to the generally restricted dietary of this dissolute young man, if not mystic, at least may be regarded as far fetched and fantastic. The milk brings him in Oedipusian relations with his mother's breast and nipples. The sausages are symbolic of the phallus and even the waffle iron is made to symbolize a pig's tits and thus play its part in carrying out the interpretation. It does not seem to me necessary to comment on this supposed illustration of the Oedipus complex, and I will leave my auditors to decide for themselves as to the good sense or nonsense of the illustration.

The study of Sophocles' tragedy is rather enlightening with regard to the extensive use of the Oedipus complex in psychanalysis. In the first place, because of the prophecy of Phoebus that the son of Laius and Jocasta would murder his father and marry his mother, his parents when he was only a few days old handed him over to a messenger, who was to take him with his limbs gyved and expose him to die in some desert place. It is to be observed that he had probably never been suckled by his mother at all. In the second place, Oedipus, whose life was saved through the kindness of a shepherd and who was adopted by the king of another country, was brought up with the idea that his adopted father and mother were his real parents. Later he became a conquering hero. On one occasion, meeting by chance his father on the highway, he slew him in resentment for what was done by the king's herald and was attempted to be done by the king. He had no knowledge that he had killed his own father. Still later he married his own mother Jocasta, the widow of the king. In other words, both the murder and the incest were accidental and had absolutely no relation to the infantile and juvenile experiences of Oedipus. It is true that the

14. Lloyd, James Hendrie: The So-Called Oedipus-Complex in Hamlet, J. A. M. A. **56**:1377 (May 13) 1911.

15. Jelliffe, Smith Ely: The Technique of Psychoanalysis, Nerv. & Ment. Dis. Monograph Series, No. 26, 1918.

story has in it both incest and murder, so dear to the hearts of the psychanalysts, but it is utterly lacking in the main elements of the freudian idea of infantile sexuality and the child-father and child-mother complex.

Under the lead of Freud, the symbolism of dreams has apparently become the chief support of direct psychanalysis, which without dream analysis would probably have soon fallen to the ground.

In profound slumber it is probable that we do not dream any more than we do in what might be termed the knockout unconsciousness which results from a blow. The dreams which can be studied directly in our waking hours, or which can be reproduced by special processes as by hypnosis and deep abstraction, represent what occurs in the predormitial, postdormitial or intradormitial states, that is, in states preceding or following sleep, or in a period of shallow slumber.

A few well known or elemental facts regarding dreams may here be recalled, namely, that they are dependent on experiences recent or remote; that they may be in their content either contemporaneous, retrospective or prospective. They are usually initiated by recent and often very recent experiences. Once initiated, the dream fantasies, through free associations may carry back in time and expand more or less indefinitely. The prospective or anticipatory content of dreams, as Peterson [16] has pointed out, has been largely neglected by psychanalysts in general, in spite of the fact that wish fulfilment plays so large a part in the freudian dream theory.

I have analyzed, or at least have studied, many dreams, not only those which I have experienced myself, but also those of others. In this study I have particularly made an effort to inquire into the dreams of persons of different types and ages. A few dreams are not only frankly sexual, but they may be both retrospectively and prospectively of this description, this fact depending largely on the contemporaneous existence of sexual ideas, tendencies and even experiences of the dreamer. Dreams as to hunger and thirst usually have their initiation in recent or contemporary sense experiences of the individual, or in the verbal or written recital of others.

My views also are entirely in accord with Peterson in statements like those which follow:

> The currents of dream consciousness would seem to be a kind of reflection of the currents of alert consciousness, a moonlit underworld of daily common life, with wider horizons as to past and future. . . .

16. Peterson, Frederick: The Psychology of Anticipation and of Dreams. Contribution to Medical and Biological Research, dedicated to Sir William Osler, New York, Paul B. Hoeber, 1919.

It would seem as though sex and hunger play a secondary part in the behavior of mankind in general. Survival and reproduction are necessary to man's advance as conqueror of his environment, but the goal of his ultimate consummatory reaction is the encompassing by his brain and mind of the planet and the stars.

The "stars" and "planet" thus metaphorically referred to include matters intelligent, intellectual and cultural.

It is the manifest content of dreams which chiefly concerns the dreamer and should chiefly concern the analyst. Symbolic meaning may sometimes be found in this manifest content, and its vagueness may be cleared away and its limits expanded by processes of hypnosis or abstraction or perhaps by appropriate methods of psychanalysis. I do not, however, believe in the "latent dream thought" theory of Freud. To my mind it is mainly an invention of the analyst to enable him to escape from his difficulties in proving his sexual theory. The so-called latent content is in the mind of the analyst and not in that of the dreamer.

The analyst reads much into dreams by direct analysis, by suggestion and by other processes. This statement is, I know, denied by advocates of psychanalysis, but the briefest consideration will show that it has in it much of force and truth. The patient, and especially the private patient, as a rule comes to the office of the psychanalyst fully aware that the doctor is a believer in a sexual theory. The patient is in a state of mind to be led and directed and cannot fully escape from the mesh. The analyst more than the patient is the victim of a powerful autosuggestion which compels him in spite of himself to search for sexual reactions, and to read these into his analytic process.

My mind has been impressed with these ideas as to suggestion by reading many of the published accounts, usually imperfectly presented, of the cases of direct and dream analyses.

These accounts of examples of psychanalysis are usually brief, but brief or lengthy one can often find in them evidences of suggestion, persuasion and domination. In one of the latest articles on psychanalysis which I have read, the doctor, first using the method of word association and supplementing this with suggestion, persuasion and bulldozing, was able to obtain an unusually distasteful story of sexual desires and experiences from an unfortunate young woman.

The method employed in direct psychanalysis and in that of dreams is a stimulus not only to the imagination of the patient, but also to fabrication. In reading one of Jung's accounts of the analysis of a precocious girl of 11, this appeared to me quite evident. It would not be too much to say that this child had the time of her life in the dozen or more interviews with her doctor.

Only a word or two can be said with regard to phallic or sexual symbolism although this subject might easily be made one for a lengthy essay. Drawing from the stories of savage life, from folklore, from myths, from early historic writings, from paintings, sculpture and architecture, from the slang of the bowery, the workshop and the farm, from almost anything in heaven above, or the earth beneath or the waters under the earth, phallic or sexual symbols have been indefinitely multiplied. Everything elongated or capable of standing, from a banana, a snake, a cane or an umbrella, to a church steeple or the Eiffel Tower, has been described as the symbol of the phallus; and every sort of an inclosure from a work-box or a basket to a cave or chasm or cañon has been found to be a symbol of the female genital organ. It is little wonder indeed that the analyst is able to read into his examination a phallic or sexual interpretation.

Although not in France, I had during the last half dozen years some opportunities to study cases of so-called shell shock, and I had much larger opportunities of dealing with these questions in consultation with neurologic colleagues who held positions in neuropsychiatric hospitals both in France and in this country, and during both the active period of conflict and the time which has elapsed since the armistice in the fall of 1918.

To the literature of functional neuroses and insanities resulting or apparently resulting from the war, I have given much attention. As is well known, I wrote the introduction to Southard's [17] treatise of almost a thousand pages on shell shock and neuropsychiatry. In my study of this work I read the histories of almost all the nearly 600 cases which it contains. So little attention is paid in the reports of these cases to freudism, jungism and adlerism and other psychanalytic schools that psychanalysis of the sexual variety seems to have been regarded as negligible.

MacCurdy,[18] although himself a psychanalyst, in his "War Neuroses" pays little attention to psychanalytic problems.

Rivers, in his preface to MacCurdy's book, says that this author when considering the simplicity of the war neuroses as compared with those of civil practice, rightly ascribes this to the fact that the war neuroses depend essentially on the coming into play of the relatively simple instinct of self-preservation, while the neuroses of civil life largely hinge on factors connected with the far more complicated set of instinct associated with sex. MacCurdy agrees with other observers

17. Southard, E. E.: Shell-Shock and Other Neuropsychiatric Problems, Boston, W. M. Leonard, 1919.

18. MacCurdy, John T.: War Neuroses, University Press, Cambridge, 1918.

that sexual factors take a comparatively small place in the production of the war neuroses, but he believes that those who have shown a lack of adaptability to the experiences dependent on sex are also liable to fail in their adaptability to the stress of warfare.

The fact remains, however, that the war neuroses in their symptomatology, diagnosis and prognosis bear a close resemblance to those observed in civil practice. They are perhaps more acute and at times more severe, but they exhibit the same phenomena physically and mentally as the functional neuroses—anxiety neuroses, hysteria and compulsion neuroses—for which Freud and his followers claim that psychanalysis is the effectual treatment. The patients in the cases detailed by MacCurdy seem to have been cured chiefly by methods of powerful suggestion, rest and physical treatment, measures chiefly employed by neurologists and alienists before the entrance of psychanalysis into medical practice.

If these war neuroses, thousands on thousands in number, were developed chiefly through fear often intensified by previous fatigue, and could be speedily cured by methods not of a psychanalytic nature, why may not the milder cases with which we have always been familiar be similarly cured? Col. Robert Armstrong-Jones and Colonel Springthorpe, in a discussion before the British Medico-Psychological Association, took a strong position against the influence of sexual factors either in the etiology or in the treatment of the many cases of war neuroses which came under their observation and supervision. These gentlemen, like others who have written on this subject, show distinctly that the instinct of self-interest or self-preservation rather than any sexual experience or conditions was the great factor in producing this war neurosis which had at its base, in the vast majority of cases, fright or terror. Psychanalysis was tried in some of the cases reported by Colonel Armstrong-Jones and failed, the cases afterward being successfully treated at the Queens Square Hospital. Why waste weeks, months and years with a method of treatment not more useful than our old-fashioned methods of rest, isolation, physical rebuilding, associated with suggestion, persuasion and the personal domination of the patient by the physician?

Ernest Jones in one of his papers on war shock and psychanalysis attempts a defense of the psychanalytic views as expounded by Freud and his followers. This shrewd dialectician is not at his best in this article throughout which he seems to beg the question. Like MacCurdy, he argues that a neurosis of war can be traced in the first place to lack of adjustment or lack of adaptation plus the more immediate exciting causes of fear of exposure, and then of mutilation and death, and then

of distress connected with the methods of life in the trenches. He does not pay sufficient attention to the subject of fatigue, to which MacCurdy gives proper consideration. He makes a feeble effort to relate war shock to sexual phenomena, but frankly admits that he was not able to do this to any considerable extent. He shows what every student of war psychanalysis knows, that the sexual element in the causation of the neuroses is overvalued.

Reference is made in the article by Jones to the question of the traumatic neuroses in times of peace, to the consideration of which he tells us, little attention has been paid by psychanalysts. I have had a large experience with these traumatic neuroses, having seen scores and perhaps hundreds of these cases, many of which have come to trial in courts of law, and in the study and treatment of which psychanalysis has rarely been used.

Not being a psychanalyst of the freudian or any other school, I can hardly be expected to discuss the benefits which come to the patient and the community from psychanalysis. One might as well be expected to treat of the benefits to nervous and mental cases derived from "mutilation therapy," as I am inclined to call the forms of physical treatment which have at their basis the mutilation, destruction or removal of various portions of the body, such as the teeth, tonsils, ovaries, sections of the intestines, etc. It is of course to the advocates of psychanalysis that I must leave the enumeration of its beneficial effects.

It is my belief, however, that the propaganda for psychanalysis has been of considerable benefit to physicians and psychologists in that it has directed their attention to the more minute and profound study of sex, and, as Jung would say, to other energic phenomena, although the study of sex problems has never been neglected entirely by the really able exponents of neuropsychiatry.

Some neurologists and alienists have perhaps shown a tendency to push aside or dodge sexual investigation, but I do not believe that this is true of the best practitioners in this department of medicine. They have steered a middle course between the tendency to include and to avoid sex problems, occasionally, perhaps because of their peculiarities of temperament and social development, tending to neglect the difficulties of the sexual situation.

With regard to the evils and dangers of psychanalysis: These are in the first place of a general character, although this term is not really as clarifying as it should be. They are the same evils as those which are inflicted on the profession and the community by the too intense concentration on mystic or semimystic subjects, such as spiritism, Christian science, divine healing, corcesterism, evangelical healing and

the therapy of shrines, like Lourdes, Knock and Stê. Anne de Beaupré. I do not hesitate to say that great evils have come to pass from the propaganda favoring these so-called forms of cure.

To my mind it is the same evil which may come and which has come from the active propaganda of psychanalytic teachings. I do not here refer to that which has been so often expressed as the moral injury to the community, but my belief is simply that which I think every rational physician is likely to hold, that much of evil and danger may come from the advocacy and practice of any irrational form of treatment.

Like every one interested in the subject of psychanalysis, whether believing in it or not, I have given much attention to the study of the question of "transference" and the "transference neuroses." It has been defined as a feeling of acknowledged sympathy from the patient to the physician, this sympathy eventually in the course of a so-called successful analysis taking on a distinctly libidinous coloring. Almost every writer on the subject of psychanalysis, like Freud and Jelliffe, seem to recognize and discourse on the evils and dangers of "transference." The broad statement which Freud makes in connection with his discussion of this subject, namely, that "if the patient has fallen in love with the physician, there might seem to be three courses open. A permanent, legitimate relationship might come to pass, though this would be unusual. Or physician and patient should at once separate and give up the work which they had begun. The third course might not seem incompatible with the success of the treatment, namely, a temporary illegitimate love relationship."

All three of these courses are disavowed by Freud and his followers for reasons so apparent that one need not here recall them, but the very suggestion of the first and third of these possibilities is sufficient to challenge the safety of such a method of medical practice. I have known of some instances in which such dangers were realized in a serious way after the process of transference had taken place, and of course the psychanalysts themselves have acknowledged the occurrence of such untoward results. Knowing as I do that the anxiety, hysterical and compulsion neuroses can be cured in a less dangerous and less objectionable way, why should we subject our unfortunate patients to this procedure?

In conclusion I might say that I believe psychanalysis, especially the freudian or sexual variety, is tending toward the discard and in another generation will have lost its hold on the profession and the community.

A CRITIQUE OF PSYCHANALYSIS*

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I have no sympathy with the argument of ridicule which Dr. Mills has used so effectively, but, as I believe, for the moment only, and to the apparent delight of a large portion of this audience. I cannot help thinking that if Dr. Mills had confined himself more closely to the text of his prepared paper, which I had his kind permission to read in advance, his criticisms of the freudian psychology would be taken more seriously and would carry greater weight. For a serious argument of reductio ad absurdum is legitimate.

It is difficult to discuss the freudian psychology without first defining accurately what we are discussing. Otherwise we shall be discussing different things, as is commonly the case. Psychanalysis means three different things:

1. It is a method of examination of the mind for the purpose of determining what factors may be at work as determinants of any given clinical condition.
2. It is a body of doctrine.
3. It is a method of therapeutics.

When we say that we agree or disagree with, that we accept or reject, psychanalysis, we must clearly understand which of these three categories we are speaking of. Let us, then, consider them separately.

PSYCHANALYSIS AS A METHOD

Psychanalysis as method of examination of the mind by the technic of so-called "free-associations": This means simply the recovery of associated memories, of memories of past mental experiences and of behavior awakened in successive associations. Having recovered the associated memories, that is, the thoughts, wishes, behavioristic episodes and other episodes antecedent to the mental condition we are investigating, those are selected or accepted as having a causal relation to the phenomena to be explained which logically would determine these phenomena if they continued to act as subconscious proc-

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*A contribution to the discussion of the paper by Dr. Charles K. Mills on "Some Theoretical and Some Practical Aspects of Psychanalysis," read at the Forty-Seventh Annual Meeting of the American Neurological Association, Atlantic City, June, 1921.

esses and if, in consequence of conflict, they made use of certain postulated mental mechanisms by which they became transformed and disguised, as "compromise formations."

This method of "free associations" is of unquestionable value for the purpose of digging into the mind and is a distinct addition to our diagnostic technic. It has, however, its limitations and its fallacies. It cannot from its very nature, for reasons I will presently give, be relied on in itself, and particularly to the exclusion of other methods, to give exact results and therefore to permit conclusions which are unquestionable. It undoubtedly, as a method of applied psychology, gives instructive insight into the mind when conservatively and intelligently employed and permits shrewd guesses as to the causal factor for which search is made. It is one mode of attack, and sometimes the only mode at our disposal, but needs to be supplemented by other methods, particularly when the problem is to determine fundamental principles and mental mechanisms. Even for a physical examination of the body there is scarcely a single technical method that can be relied on to give absolutely reliable diagnostic conclusions: auscultation, percussion, the thermometer, the clinical symptoms, all supplement one another for the purpose of diagnosis, and the findings of each require to be interpreted. Even a positive or negative Wassermann reaction is not absolutely diagnostic of the presence or absence of syphilis. If errors attend the diagnostic technic in physical conditions and the findings of each and all methods require interpretation, how much more must this be the case with any technical method that attempts to analyze the numerous and subtle factors involved in mental motives and behavior?

The errors liable to result from the method of "free-associations" are due: (1) to the necessity of depending on interpretations and inferences to connect an antecedent experience with a present phenomenon and, for the purpose of interpretation, of postulating very debatable mental mechanisms to account for the determination by a selected antecedent experience of a present symptom or phenomenon, (2) to the fact that by the use of interpretations very different antecedent experiences can be found to be the logical causal factors of the phenomenon in question.

When in interpreting a given phenomenon, as for example, to take the stock phenomenon, a dream, we have to apply one or more of various theoretical mechanisms, such as conflict, repression, displacement compromise, disguisement (to avoid a hypothetical censor), inversion, transposition, regression, projection, introjection, transference,

dramatization, condensation, sublimation, fixation, compensation, etc. —when we have to do more or less of all this in order to connect an antecedent experience logically with the dream or other phenomenon under investigation, it is obvious—at least so it appears to me—that the method falls far short of having that exactness which scientific procedure requires, and becomes a source of a large number of possible errors.

It is impossible for a method of this kind to be reliable and it must give different results according to the intellectual points of view of the interpreters and their differing personal evaluations of the data. But, curiously enough, it is by this very method that the conceptual mechanisms, doctrines and postulated principles of mental activity adopted by psychanalysis, have been "proved."

Having thus been proved by "free-associations," these postulates are then in turn employed to prove as determinants the selected associations in the psychanalysis of a given case. This seems to me, as my mind works, very much like a very vicious circle.

Here I am led to point out that in the evaluation of psychanalysis as a method we must distinguish between its use to determine the laws and mechanisms of the mind and its use in applied psychology to discover the particular origin and causes of the psychologic phenomena in particular cases. For the former purpose it is, for reasons I have mentioned, inadequate and unreliable, just as digging holes in the ground is a very inadequate method for determining the principles of geology. But just as digging holes is a very useful method for a mining engineer to employ to determine whether a gold bearing geologic formation is continued deep down in the bowels of the earth, according to geologic principles established by other methods, so digging into the mind by psychanalysis is a method of great value to obtain information regarding what is contained or going on therein. It is a method of applied science rather than of science itself. In the hands of unbiased and experienced clinicians it is capable of giving valuable results provided those results are not dependent on interpretation through hypothetical concepts and doctrines, themselves of a questionable nature. Too often the results reached are only of a greater or less degree of probability or even possibility.

But I would insist that to demonstrate the mechanisms and processes of the mind and the psychologic principles underlying its phenomena, including in the latter its aberrations, such as phobias, compulsions, hallucinations, delusions, affective conditions, stupors, amnesias, deliria, etc., and the phenomena of conflict in general, we

need more exact methods, comparable at least to those employed in physiologic research and adequate to give relatively exact results.

In illustration of this point I may be permitted to cite an article by a whole-hearted psychanalyst whose manuscript I have had an opportunity to read in advance of publication. The author criticizes, and justly as I think, the psychanalysts for not having analyzed more adequately the repressing force in conflicts and having confined their attention almost exclusively to that which is repressed. He then proceeds by the method of free associations to analyze the former and arrives at the generalization that the repressing force is a wish to please the mother or father and this wish is also a sexual one. So that the conflict is between two sexual wishes and the symptoms are a compromise between them! This seems to be going some. The poor "censor" must indeed have a hard time of it!

PSYCHANALYSIS AS A BODY OF DOCTRINE

We come now to psychanalysis as a body of doctrine. We have to deal with the concepts which have been reached by the method of free associations and postulated as fundamental principles.

Let me say at the outset that there are two principles which, in my judgment, must be accepted as contributions of the highest importance to our knowledge, and for these we are indebted to the genius of Freud. One of these is the doctrine or the theory of repression and the other is that of conflict. They are intimately related, for it is through conflict that repression is induced. Before speaking of these I feel called on to say a few words about the subconscious, for this underlies and is the basis not only of the doctrines of psychanalysis but of the theories of psychopathology of every modern school of research. The theory of the subconscious is not novel, nor was it originated by Freud, though that seems to be the impression of many writers on psychanalysis. As a philosophical concept of the unconscious, of which I will later speak, it dates back as far as Leibnitz and Kant, and, later, Schelling, Herbart and Hartmann, the last having evolved it into a biologic and metaphysical system. Freud converted it into a debatable doctrine peculiar to psychanalysis. It remained for Pierre Janet, in France, and Edmund Gurney, in England, to take it out of the field of philosophical speculation and demonstrate by the inductive method its concrete reality as mental processes underlying certain psychologic phenomena. This real pioneer work, in 1886-1887, laid the basis, by the use of experimental methods, for every theory of modern psychopathology that makes use of subconscious processes. It

was comparable in the field of psychology to the demonstration of radio-activity by physicists as intra-atomic processes essential for the explanation of certain physical phenomena. In using the word "demonstration" it must not be lost sight of, however, that both radio-activity and subconscious processes are logically only theories, though as theories they have attained the highest probability of truth.

Dissociation with the formation of subconscious processes that, motivated by their own emotional impulsive force, take on autonomous activity and induce psychologic and physiologic phenomena, had been demonstrated previously, as I have said, by Pierre Janet and Edmund Gurney, working independently, and their conclusions were confirmed (1889 to 1890) by Alfred Binet, William James, Max Dessoir and myself. But later Freud with Breuer (1891) discovered and applied the principle and mechanism of repression to explain the dissociation in the psychoneuroses. This although only a particular application of the dynamic principle was a marked advance. It is Freud's great contribution to our knowledge of the psychoneuroses. Its importance can scarcely be exaggerated. Nevertheless, it must not be forgotten that dissociation may be effected by simple failure of normal inhibition without repression from conflict, though this is not recognized by psychanalysts.

The motivation, however, of the succeeding subconscious processes became another problem, and the debatable doctrine of the repressed sexual wish, and particularly of an infantile sexual wish, was insisted on by Freud as furnishing an almost universal motive, or urge, impelling these processes to fulfil a sexual aim.

Without further discussion of this wearisome question, I want to point out only that the valuable theory of repression does not require the acceptance of the doctrine of the sexual nature of the conflict and of the repressed subconscious processes. That is an entirely different problem. It is to Freud's credit, too, that the basis of his investigation was the search for the "Why." No complete solution of the problem of repression, dissociation and subconscious activity and no complete understanding of the resulting phenomena can be attained without answering the question of the "Why." How and That are inadequate explanations. But while the freudian psychology has been most stimulating in directing a search for the "Why," its own answer was and is, in my judgment, radically wrong and has had a most baneful influence.

Repression, as I have said, necessarily implies and requires conflict.

The recognition of mental conflicts and their disturbing effects on the individual is as old as literature itself, but we are indebted to

Freud for the recognition of them as factors in the mechanism of repression and the important part they play in the maladjustment of the individual to the environment and the induction of the psychoneuroses. This does not mean, however, that we must necessarily accept the universally sexual nature of the conflict, or the very special mechanisms postulated through which it is conceived that the subconscious process fulfils its aim, or the symptoms are compromise-formations. After recognizing and giving due credit for the theories of repression and conflict, I must think that the greater part of the remainder of the doctrines of psychanalysis are not only unsubstantiated, but cannot be reconciled with a large body of facts which have been gained by other methods of research and which must be taken into consideration. Furthermore, nearly all the facts which have been advanced as the result of psychanalytic methods are open to other interpretations than those which the fundamental doctrines of psychanalysis postulate.

It is, of course, impossible in the time at my disposal to consider all these debatable concepts, but I may take one or two of them as examples.

Let us take the concept of the "unconscious," as used in the freudian sense. According to the true freudian psychology, a marked distinction is made between two classes of unconscious processes, namely, the preconscious, or thoughts which once were in the domain of consciousness and which can be recalled by the ordinary mechanisms of memory; and the unconscious proper which is not only considered to be inaccessible to direct introspection, but also to be essentially different in kind from the processes of the foreconscious. To reach the unconscious proper requires technical methods of procedure, such as psychanalysis. The unconscious also has marked characteristics which distinguish it from the preconscious, and it alone induces the phenomena of the psychoneuroses.

Before speaking of this I would point out that this division and distinction of subconscious processes is not only not confirmed by other methods of research, but also is distinctly contradicted by the findings so reached. Such a distinction and classification is therefore purely arbitrary and dogmatic. Although it is essential to the pure psychanalytic psychology, I think it can be clearly shown that subconscious (that is, unconscious) processes that cannot possibly be revealed, except through special technical procedure, have been antecedently conscious (that is, preconscious) and induce conflicts and symptoms of the characteristic classical kind. It is true that some latter day psychanalysts have given up this classification of the unconscious: neverthe-

less, we have been told over and over again that it has been "proved" by psychanalysis and is still held by Freud in his most recent publications.

As to the character of the unconscious proper of Freud, it has been stated axiomatically by an earnest group of students in Europe and America that unconscious processes are of a specific kind, have a specific origin, have specific characteristics and exhibit a specific behavior. According to this theory, the unconscious embraces only processes that:

1. Have been repressed from or kept out of consciousness because intolerable by reason of their unmoral, unsocialistic and other characteristics;

2. That for the most part never have entered awareness and therefore have been unconsciously and automatically kept apart;

3. That are essentially of infantile origin and nature, the splitting of the mind into conscious and unconscious regions "having taken place in the earliest part of childhood life, probably in the first year";

4. That involve the crude primitive instincts;

5. That are predominately sexual in character; and

6. As they, necessarily, like all processes, are dynamic, that they are sexual wishes.

To quote the exact words of one of the leading and most qualified expounders of this school, "the unconscious is a region of the mind, the content of which is characterized by the attributes of being repressed, conative, instinctive, infantile, unreasoning and predominately sexual."

I have no intention, least of all desire, to enter into any disputatious discussion of this theory. I want only to indicate the point of view from which I approach this problem and to record my dissent from any such limited theory of the subconscious. Without denying that some subconscious processes may have these characteristics (reserving an exception, as the lawyers say, to the extreme infantile origin postulated), to hold that all, or that subconscious processes generally, are of the character stated is to take a far too narrow view of the phenomena. One may say, if one likes, that he will call unconscious only those which have the characteristics mentioned and so arbitrarily define the subconscious. But that does not alter the fact that there are vast numbers of processes that are subconscious and that have not any of the six characteristics contained in the definition and particularly of repressed sexual wishes, or even of having been repressed at all, and that these take part in conflicts and repression with resulting phenomena. The statement contained in the theory is simply not true. One might as well define men (*qua men*) as criminals, or even as

sexual criminals, repressed in jail, and disregard all other men who are not criminals and not in prison.

And so this conception of the subconscious is not true, not only because it is not confirmed by other methods of research at our disposition but, as I have said, it is contradicted by the findings of the latter.

The fallacy which has led to the theory is, to my way of thinking, as I have already said, due to the inexact and inadequate methods of research of which I have spoken. This doctrine of the subconscious can only be accepted by those who have not had a long training in the technic of the methods of psychologic investigation other than those of so-called "free associations" and who have not a familiarity through actual experience with all the facts as revealed. The validity of the freudian doctrine of the unconscious is of the utmost importance because on it hang all the laws and the prophets.

Let us take the doctrine of the "libido," which has been one of the fundamentals of psychanalysis, although there is a tendency today on the part of some psychanalysts either to modify it or to discard it entirely, notwithstanding that it has been supposed to have been indisputably "proved." The doctrine of the libido postulates a concept of a free energy, sexual in character, derived from the primitive sexual instinct, capable of transference from one set of processes to another, and which in its urge motivates and determines through various very special mechanisms psychoneurotic symptoms and other mental and bodily phenomena. Such a libido, sexual in character however much it may be camouflaged by philosophical considerations, is the source of all energy in the individual, though it may be "sublimated" beyond recognition in the higher moral, social, ethical and esthetic ideals and activities.

This conception of the libido entirely disregards the theories of the instincts and the data on which they are based as advanced by biologists and psychologists who are occupied in investigations in other fields of research.

The instincts and instinctive processes and the behavior to which they give rise have been the object of study to earnest students of human and animal behavior, and their investigations have presented important results. There is by no means, it is true, an agreement of interpretation, but the theories advanced are impressive and must be taken into consideration.

All this work is entirely ignored by the freudian school of psychanalysts and until recently by all psychanalysts; and yet some of the interpretations of instinctive processes derived from such studies offer

explanations of human behavior which are entitled at least to consideration. None of these theories may be true but one or more of them offer explanations which are as probable and adequate as that represented by the libido.

What shall we say, for instance, to the theory of McDougall, which has engaged the earnest attention of psychologists and which finds in the urge of many instincts the motive forces which determine normal behavior, a theory by which the phenomena of conflicts can be readily explained? This theory, as well as others, is fully as dynamic as that of the freudian concept.

And if I may be permitted to strike a personal note without injecting into this discussion theories I have long advocated, I would refer to my own observations and experimental studies of conflicts and their resulting phenomena, carried on now for twenty years and more by other and, I believe, more exact and reliable methods than that of "free-association," although the latter has been freely made use of as a supplementary control. These studies have led to conceptions of motivating forces and mechanisms which wholly contradict the philosophical doctrines of the "libido" and give very different interpretations of the phenomena of conflict.

The freudian concept, it is true, makes use of the primitive instincts, but in a subordinate way, and makes the libido of the sexual instinct the predominant motivating force. The psychologic and biologic concepts, on the other hand, give to each one of them its just and equal part in the determination of mental and bodily behavior.

Again, take the doctrine of symbolisms. That symbolisms occur as mental phenomena no one questions, but the universality of specific symbolisms occurring in all minds, in all places, and in all ages, is, to my way of thinking, an extravagance which passes the bounds of inductive science. The postulation of a symbolism may be necessary to explain the connection between a mental phenomenon and some selected antecedent experience in order to interpret the latter as the causal factor, in accordance with one or other freudian doctrine; but it does not follow that the causal factor may not be some other equally well demonstrated one utilizing some much simpler mechanism. As a matter of fact, I believe that when such phenomena can be submitted to other technical methods of investigation, it will be found that in the great majority of cases we can find evidences of subconscious processes which are subject to an entirely different interpretation than that requiring a symbolism.

So I might run through the various doctrines, concepts, or hypotheses, call them what you will, of psychanalysis, and I believe that

evidence could be presented that would equally well support other interpretations than those which make up the body of doctrine maintained by psychanalysis.

Similarly, one might run through the various mechanisms which have been postulated in explanation of amnesia, misspelling, miswriting, misspeaking; of misplacement, inversion, projection, introjection, transposition, etc.; and through the theories of compromise, disguise, regression, and so on, and if one should do this, I think it could be shown that although there may be a certain aspect of truth in each of them in certain specific instances, the use of them as universal formulas to be applied in every instance, not only is a fallacious method but leads to conclusions which are not confirmed by the findings by other methods of research. Amnesia, for instance, may at times be due to the repression of an intolerable memory, or by a wish to forget or some other wish; yet at other times, and probably in the great majority of instances, it is brought about by entirely different mechanisms. Thus it may be due, not to repression in the freudian sense, but rather to a neutral condition of disinterest and perhaps still more often to inhibition by another interest of greater intensity, although both are motivated by palatable aims. In other cases forces using still other mechanisms, which I cannot go into, have brought about the amnesia. But in truth the problem of amnesia is subordinate to the fundamental problem of why we remember at all.

Summing up this analytical criticism, I would say that the great majority and general body of freudian doctrines reached by this method of free-association have not only not been confirmed by other and more exact methods of research, but have also been contradicted by the results so reached.

The failure of the psychanalytic method is due to the fact that the findings require and depend on too elaborate, intricate, and, however ingenious, debatable interpretations, which themselves depend on debatable theoretical mechanisms and forces, and the method itself is not only inexact and inadequate but open to artefacts of the most subtle kind, particularly in the selection of the data. The conception of the subconscious and the theoretical structure built upon it approaches more nearly a philosophy than a science, and with philosophy we are not concerned.

PSYCHANALYSIS AS A METHOD OF THERAPEUTICS

Time will not permit me to more than touch on psychanalysis as a method of therapeutics. That cures may be effected by this method may be admitted as by any method of mental therapeutics whether of

Christian science, faith, suggestion, amulets, charms, reeducation or what not. Successful therapeutics is no proof of the truth of the concepts accepted as articles of belief. Disbelieving, as I do, the fundamental concepts of the freudian psychology, I believe the orthodox method of psychanalysis as practiced by the out-and-outers, is distinctly wrong in principle and meretricious in practice, using the latter adjective in its strict etymologic sense. In too many cases it is degrading to the personality, produces harmful results and is indefensible. Reeducation I advocate, but not of the freudian kind.

THE INFLUENCE OF THE FREUDIAN PSYCHOLOGY ON PSYCHOLOGIC AND PSYCHIATRIC THOUGHT

After having thus criticized the psychanalytic method and the validity of the doctrines, it would be ungenerous not to bear a well deserved tribute to the influence of the psychanalytic school on contemporaneous thought. I regret that the time at my disposal will not permit me to do full justice to this aspect of the subject. There can be no question that Freud, supported by a large group of enthusiastic followers, has compelled the attention of psychologists and physicians and forced them to take heed. Previously scant attention or courtesy was paid to any researches in dynamic or subconscious processes. No better evidence of the change can be adduced than this very discussion —the presentation of the topic of psychanalysis by a distinguished neurologist as the leading subject on the program of this Association today. Time was when any paper on the functional or dynamic interpretation of the psychoneuroses and allied problems was practically taboo in this society of neurologists and psychiatrists, as I can bear witness from personal experience. The older members will remember this uncultured attitude of mind. It was a resentment of new doctrines, of functional conceptions as opposed to the traditional organic conceptions—a Will to Disbelieve. The experience with the psychoneuroses in the great war has been the greatest factor in breaking down this prejudiced attitude. For, as I believe I am right in saying, there is a unanimity of agreement on the part of those who were engaged in the study and treatment of the war neuroses and psychoses, whether in England, France or America, that their pathology is to be found in dynamic, functional motivations and mechanisms and not in organic defects. This point of view and this conception are not novel, but the psychanalysts compelled attention when it was previously refused and forced the study from the point of view of the freudian psychology.

This, to my mind, is the great gain for which we must be thankful to Freud—the acceptance of the dynamic approach and the dynamic

conception of aberrations of the normal personality however manifested. It is the search for the "Why" which is the final step in dynamic pathology. Undoubtedly there is at present a tendency to go too far in the search for functional causes and mechanisms and to overlook the conditions of organic disease. Sometimes one would think that there is no such thing as organic disease to consider. We must keep at least one foot on the ground and not soar all the time, or too high.

While the war forced the dynamic point of view on a reluctant profession, it has also given the coup de grace to many of the freudian concepts and in particular to the sexual wish as the universal determinant of the psychoneuroses. In other words, the freudian doctrine of the "Why"—of the libido and the unconscious sexual wish—broke down as a result of general experience with the war neuroses. Nevertheless, the main and important point is that neurologists and psychiatrists, while far from being in agreement as to the particular causal factors, mechanisms and processes that produce the symptomatic phenomena of the psychoneuroses, generally recognize that the explanation of the latter is to be found in the conflicting instinctive attitudes, strivings, impulses and reactions of human personality.

But what shall be said, on the one hand, of the mental processes of those who, motivated by the Will to Disbelieve, have steadfastly refused even to listen to any such explanations or weigh the evidence on which they are based; and, on the other hand, of those who, motivated by the Will to Believe, regardless of the canons of logic, of conflicting facts and inadequate methods of research, untrained in scientific methods of precision and without a comprehensive background of knowledge of the great store of psychologic phenomena accumulated by experimental psychology, have accepted as proved the excathedra dicta of a psychanalytic master and become devout, faithful believers in doctrines which have never at best been more than working hypotheses, if not mere philosophical speculations based on selected data? In these two types of mind we have a psychologic phenomenon equaling in interest those involved in the problems of the psychoneuroses.

When I was a boy I read in an essay by the French critic Sainte Beuve a sentence which has always stuck in my memory and which I like to think has influenced for good my mental tendencies. The subject of the essay was a modern miracle called the "Holy Thorn." The sentence ran something like this: "Some people see in this miracle a special intervention of Providence; but I can see in it only the humiliation of the human mind."

DISCUSSION ON PSYCHANALYSIS

Dr. Sidney I. Schwab of St. Louis said that there was one aspect of the subject with which he was somewhat familiar and that was the contact which psychanalysis had in the treatment of the war neuroses in the place where they happened, from the time that America entered the war until after the armistice was declared. An opportunity was given to him to test out as much of the psychanalytic procedures as could be carried out under conditions of actual service. From his personal experiences he was led to believe that much of Dr. Mills' objection to it as a therapeutic principle of general application in the neuroses was true. Dr. Schwab approached this testing without the least prejudice against the freudian psychology but rather much inclined to favor it. The war neuroses had proved the ineffectiveness of the psychanalytic method of treatment based on the assumption of its sexual etiology. In literally thousands of cases the etiologic factors had nothing to do with the instinct of sex but had a great deal to do with a more powerful instinct—that of self preservation. He stated that the experience with the war neuroses contributed this fact and there could be no denial of its importance in any discussion of the psychanalytic procedure or of the freudian psychology as applied to the neuroses.

On the other hand, a good deal of the freudian mechanism was absolutely proved to be a definite and crystallized thing in the building up, development and organization of the neuroses.

Dr. Schwab believed that these two things were well worth discussion and debate. That which the experience with the war neuroses has shown to be valuable and true should become the property of every farseeing neuropsychiatrist. The things that this experience had proved to be untrue should be discarded with all their nomenclature and with all their intricate and mystic complexity.

Dr. John T. MacCurdy of New York said that he was afraid that if the audience expected any gladiatorial exhibition from him it would be doomed to disappointment.

In following what Dr. Mills had said he endeavored to separate the various elements, and found there were perhaps three things that were to be considered. The first was definite argument, all of which was directed against psychanalytic theory in its different schools. As to that Dr. MacCurdy admitted that there was not a single objection which was not an echo, usually in different terms, but nevertheless an echo of difficulties that he himself had met in assimilating the various theories that had been put out by Freud and his followers. It was perhaps not unnatural that Dr. Mills had arrived at somewhat different conclusions than himself, considering the variance of their sympathies.

He refused to enter into the minutiae of these different points as that would take, literally, many hours and briefer discussion would be futile.

The second element was that Dr. Mills expressed certain personal opinions on which Dr. MacCurdy made no comment. He felt responsible for his own opinions and would not presume to criticize another person's opinions which were not based on an argument.

The third and most important element seemed to the speaker to be the general drift and implication of the paper, which was that psychanalysis was a rather ridiculous system of therapy or of psychopathologic theory. It was

so full of logical errors as to be absolutely useless, and presumably therefore should be thrown into the discard. In fact, Dr. Mills finished with a statement that he believed that the psychanalytic movement would run its course and another generation would know it no more.

Dr. MacCurdy reminded the audience that he agreed in essence with many of the criticisms directed against psychanalysis but considered it important to realize the situation with which we are faced. The word, psychanalysis, which fifteen years ago was almost unknown, is today on every one's lips. The commoner terms of psychanalysis had become words of common speech. Patients went to physicians throwing technical terms at their heads. Psychanalysis was allegedly being practiced by laymen, and it seemed impossible to prevent this. If the majority of the medical profession viewed this spread of psychanalysis with horror, it was nothing compared to the frenzy into which it threw the more conservative men who were really interested in psychanalysis.

He thought it would be profitable to consider the actual factors that had possibly gone into the making of this situation. The first one should be recognized frankly. Psychanalysis held most of its popularity in and outside of the profession through what might be termed a pornographic lure. This was an expression not merely of the innate and perennial human interest in sex, but was also part of the general wave of unrest which culminated in the World War. There were, however, a good many evidences of people kicking over the traces of convention for some years before the war began. When psychanalysis appeared it provided a lovely pseudoscientific justification for allying ourselves with the brutes. The argument was, "We must not deny our kinship with the brutes, psychologically, any more than we do anatomically and physiologically. Therefore, if we have these instincts, we are false to ourselves if we do not express them, and express them openly"—an argument which was based, of course, on the fundamental fallacy of lack of recognition of the fact that there was such a thing as human specialization, which involved a definite inhibition of many of these instinctive tendencies in so far as they might be directly expressed.

The second factor dealt with the mystical tendency which Dr. Mills had so emphasized, which was probably much commoner than was realized. It attracted a great many people to psychanalysis.

A third factor, of minor importance, that had something to do with its adoption as a fad, was that it provided a vocabulary with which to slang our friends. A few years ago one could not call his companion a thief or liar without causing some disturbance, but now one could talk about his complexes—a very convenient thing for a good many people. There was a large amount of this meretricious practice in the so-called Greenwich Village type of psychanalysis.

The fourth and most important factor to be considered was the actual achievement and scientific basis of psychanalysis. When Freud's psychology appeared there was a hiatus in our general biologic conception of man; Darwin and his followers had worked out his anatomic and physiologic evolution, but no one had done anything effective in studying man's psychologic relationship to the rest of animal creation and his mental specializations. Now psychanalysis, in Dr. MacCurdy's opinion, represented an attempt (he wished to stress the word "attempt") in that direction, and in so far as that attempt was sincere, in so far as it had been productive of results, there was a sound scientific basis for psychanalytic work.

He wished to raise the question of the exact medical status of psychanalysis in this connection. What did it offer as a method of therapy? As Freud and all psychanalysts had said, it was not a panacea for all psychologic ills. He thought it might be well for him to say what he considered to be the requisite for a successful analysis. The first thing is that the patient should have sufficient intelligence to grasp properly abstract ideas. The second thing was that this intelligence should have been directed, or was capable of being directed, to the pathologic nature of the symptoms and behavior of the patient. One could not cure a patient of something that he did not know he had. The third thing was that there should be a really potent desire to be well and to change the deleterious tendencies recognized by the patient. If those three qualifications were possessed, or could be fostered in a patient, he believed that such an individual could be cured and cured completely by psychanalytic procedure.

Then an important thing to consider was the "psychanalyst" himself. There were now many people calling themselves psychanalysts who were entitled to use the term because there was no copyright on it and there was no legislation in this matter, no licensing. What made a good psychanalyst? In the first place, he should be a medical man. Dr. MacCurdy doubted that he had ever had a single interview with a patient in which he did not quite consciously make use of his definitely medical education, and he did not see how that could be dispensed with. The second point was that the analyst should have a thorough psychiatric training. If one traced the scandalous stories one heard of ill-treatment in analysis (Dr. MacCurdy excluded the work of irregular practitioners, but referred to that of good ethical medical men), it would be found in practically all cases that the patient making the complaints was suffering from a definite mental disease. The analyst lacking psychiatric training had not recognized this fact, through ignorance, had talked of transference to his patient who had translated it into ideas of definite persecution at the hands of the analyst.

Dr. Daniel J. McCarthy of Philadelphia said that he was not prepared to accept the vague interpretations of symbols and symbolisms of the psychanalysts as facts, or even near facts, that one could scientifically, safely and continuously reason from to scientifically accurate conclusions. The objection to applying the freudian method to a study of the neuroses, the psychoses and psychoneuroses, is that psychanalysis, as practiced, does not lend itself to criticism by the same rules that apply to laboratory investigation in chemistry, pathology or bacteriology. What scientific workers have a right to demand is that theory should be based on facts, and the facts firmly established, dissociated from theory. Therapeutic results are no criterion of the accuracy of a theory. So much of suggestion, of autosuggestion, of the influence of the personality of the operator, creeps in, that we can no longer tell in the final analysis how important a part each plays. If such were true, Christian science or osteopathy could well point to cures to bolster up the accuracy of the theory. If in the study of a mooted point like symbolism, instead of isolated, interesting cases, a study of one thousand, or ten thousand cases of an isolated disease, like praecox, could give some uniformity of result of the dream content and its symbolization, this would approach a fact and would carry conviction. And so with the other mooted points in reference to the psychanalytic theory.

Assuming that the study of the war neuroses demonstrated that the sexual basis of all the neuroses was wrong, this did not mean that the psychanalytic

theory had no value, but that it had quite as much value with only a relative percentage of cases of sex origin. The fundamental principles of repression and complex formation and conflict remained as a valuable method of study and treatment of these and other cases.

It was early demonstrated that the Ehrlich side chain theory was scientifically incorrect. Ehrlich promptly admitted this. Notwithstanding this, there never was a principle enunciated, not even excepting the Darwin theory, that did more to stimulate and activate scientific work than the Ehrlich theory. The theory, though wrong, was a tremendous stimulus to immunology and therapeutics, to an understanding of infectious processes.

The same can well be said of the freudian theory. Nothing has done more in our time to stimulate a study of the psychoses and psychoneuroses than this theory. Right or wrong, it has awakened a new interest in psychiatry and functional neurology; it has infused a new life into dying subjects. Apart from this, the great value of the freudian theory was to change psychology from an academic discussion of threshold sensations, time reactions, word associations and a philosophic discussion of the component parts of the mind to a real live study of the determination of conduct and its disorders. In medicine, it has taught us the value of properly taken histories of our patients, of the application of psychology to the interpretation of the influence of sex and sex matters in the life history of the individual. Those who have stood adamant against this theory must indeed be blind if they have not seen in their daily work many examples of the freudian mechanisms at play in the amnesias and hysterical disorders of conduct. Dr. McCarthy said that he did not see how any one who studied the psychoneuroses in private practice, in institutions and during the war could have avoided recognizing that a fairly definite percentage of cases belonged without question to the freudian conception of the psychoses and psychoneuroses; the physician who did not see this disregarded what was evident to any intelligent individual.

It appeared to him that there was an intermediate group of cases in which the sexual influence was a definite influence, but in which other physical and visceral or other psychogenic factors were at play.

There is a fairly large group of cases in which physical factors, focal infections or reactive endocrine disturbances are the cause of mental dissociation and disease, in which the sexual factor is of little or no significance.

If in studying these cases and in analyzing and classifying them, a physician or institution closed the mind to the value of the psychanalytic method and theory, a valuable aid to the approach and treatment of a very wide group of cases was lost. It did not mean that this was the only method of approach or treatment of the psychoneuroses. While Dr. Schwab, at La Fauche, used the freudian method of analysis and based his treatment on the theory of conflict at the Neurological Institute at Vichy, the treatment of the psychoneuroses was based on the principles laid down in the Mitchell school, i. e., the treatment of the human machine as a whole, giving due weight to psychogenic factors, but the various physical and visceral factors were never neglected and the reinforcement of the will, suggestion, and reeducation were employed as a therapeutic agent. The application of the principles of pathologic psychology was used from a somewhat different angle than that employed at La Fauche. Dr. McCarthy said that he thought the younger men in both groups could testify as to the efficacy of both methods.

Dr. McCarthy thought that the freudian mechanisms were not going to pass away like many of the fads of medicine of the past, but that they would

become in the future a much more important, if better balanced, part of neurology and psychiatry; that the younger group of men growing up into this atmosphere could not help but be imbued with the freudian terminology and its mechanisms and pathologic mental states.

Dr. George H. Kirby of New York expressed the view that there would be general appreciation of Dr. Mills' suggestion that the time had come to undertake the discussion of the issues of psychanalysis more on their merits and less on our emotional reaction to them. Dr. Mills in his presentation spoke of psychanalysis as if it were a fixed system, with settled formulations and a definite technic—a procedure that had been in use sufficiently long for one to draw conclusions as to its real value. Dr. Kirby felt, however, that developments in this country had been far from uniform, both in the theory and practice of psychanalysis. It was true that the original formulations of Freud had been faithfully followed by a few, but, on the other hand, there had been from the first an independent attitude on the part of many who had declined to be bound by a rigid dogma. It will be found that in practically every center of psychiatric research in this country there has been a notable departure from, or a modification of, the freudian doctrine with the result that the developments of recent years show a marked divergence from the original freudian formulations. The broader psychobiologic conceptions now current in American neuropsychiatry differ not only in form but also in principle from the freudian doctrine. He stated that this had been forcibly emphasized at the last meetings of the American Psycho-Pathological Association.

The speaker did not wish to disparage the constructive value and stimulative influence of the Vienna School—this should be acknowledged by all. But it seemed to him that much of what Dr. Mills found to criticize was already admitted as untenable and to a large extent unessential to the acceptance of the more fundamental principles established through the psychologic, or more precisely the biologic, method of approach to the study of the neuroses and psychoses.

Dr. Kirby said that pansexualism did not need to be dwelt on, as few would maintain that the sex instinct supplied the only dynamic force to be considered in the development of the neuroses and the functional psychoses.

One will readily admit that the principle of symbolism uncritically applied opens the way to fanciful interpretations and that it has often occurred; but the fact that this has been done is no reason for doubting its value. In the study of the trends of the psychoses, one will often find that very little interpretation is necessary in order to understand the meaning of the peculiar ideas expressed, as, for instance, in schizophrenia. The Oedipus situation will frequently be found in these cases in almost undisguised form, and it is not necessary to consider the rôle of suggestion as one has to make merely a careful study of the patient's spontaneous utterances; these will often tell the story themselves.

Dr. Kirby discussed one point mentioned by Dr. Mills in his abstract which was not dwelt on particularly in his presentation, namely, that psychanalysis has proved a failure in psychiatry. From the standpoint of the results of treatment of institutional cases, the assertion may appear to have some justification. He said that few claims had been made as to the cure of constitutional psychoses or functional psychoses through psychologic analysis. On the other hand, the speaker felt that not a single public institution, and few private ones, for mental cases in this country were equipped with facilities and personnel to investigate adequately and treat patients. Nevertheless, several studies have

appeared, chiefly of an experimental kind, which show that even chronic functional psychoses are often greatly improved or that even a social recovery is obtained by a careful psychologic investigation of the case, accompanied, of course, by appropriate efforts at rehabilitation.

Considered from the point of view of a method of study and investigation, Dr. Mills' statement that psychanalysis was a failure in psychiatry was to the speaker's mind entirely unjustified. There can be no denial of the fact that the psychologic investigation of the psychoses had thrown a flood of light on the development of mental symptoms in general; their origin and meaning to the patient; their significance in the general clinical picture had pointed to the forces which lie back of the development of the psychosis itself. In other words, this method of study has enabled us to see more clearly that mental disorder is best expressed in terms of a psycho-biologic reaction to life situations; has shown us that mental reactions can never be understood, even those accompanied by organic disease, by studying brain processes as such. We must know the life experiences, the conscious and unconscious conflicts, the aspirations and wishes of the individual and the reactive tendencies of the personality as a whole. The speaker said psychologic analysis contained nothing suggestive of mysticism nor was there anything unscientific about it. It was merely a way of getting at the facts which experience teaches are of fundamental importance in the understanding of abnormal mental states.

Dr. Charles Macfie Campbell of Boston said that from the point of principle he wanted to bring up the subject of the mode of presentation. Dr. Mills, with his large experience, had thought it fitting to use the tone of ridicule. He judged therefore that he considered the whole work, taught by what is widely called the freudian school, so dangerous that any weapon was justifiable. On the other hand, it seemed to Dr. Campbell that the introduction of such an attitude prejudiced the whole subject and made it extremely difficult to carry on a discussion.

Most physicians have known that considerable help can be given to nervous patients by sympathetic talks, and Darwin in his "Life and Letters" told how his father got beneath the physical complaints of women to real disturbing factors of their life.

As to the degree to which examinations are carried, in some cases no doubt after a brief discussion with the patient, difficulties could be discovered which might be easily eliminated by a frank talk. In the concrete case Dr. Campbell wondered whether the principles of the method of attack adopted by Dr. Mills were so fundamentally divergent from those of the psychanalyst, of the pupils of the freudian school. Did he not also try to understand the determinants of the symptoms presented by the patient? Dr. Campbell asked whether Dr. Mills realized that he could not find out all the relevant facts in the first interview. Dr. Campbell asked also whether Dr. Mills accepted the fact that in the determining factors of our life there are few elements more important than the early influences of the home and our relationships to our parents.

Simpler terms might be used, stating that the child has had early antagonism to the restrictions of the father; it might be said that he had an undue emotional dependence on the mother, but if another person chose to use the term "Oedipus complex," Dr. Campbell thought that he should be allowed to use his own language.

When Dr. Mills said, however, that all of the discussion of the unconscious was nothing more than a reference to what had already been presented much better under the term of heredity, Dr. Campbell wondered if that was not exaggerating the contributions which heredity had made to psychology. To his mind studies on heredity had contributed a modest amount to the knowledge of the complex determinants at work in the psychoses.

Dr. Campbell asked whether there was any psychiatrist studying intensively the deteriorating psychoses, who was not wittingly or unwittingly using the principles emphasized by Jung in his "Psychology of Dementia Praecox," based on the freudian psychology.

Some people are interested in the causes of cancer; others are interested in the treatment of that disease, and so with psychopathology and psychotherapy a person ought not to be abused because he had not made an advance in the field in which he was not working. The value of the contributions to psychology of the freudian theories is not so much along therapeutic lines, as along lines which are going to be of profound influence with regard to education.

Dr. Joseph Collins of New York stated that his qualifications for participation in the discussion were that he had carefully observed the freudian movement for twenty-five years, and for ten years had been an active practitioner of psychanalysis.

He had no intention of attempting to point out in the brief time allotted him how the freudian doctrine was rooted and nurtured in neoplatonism. Any one who wished to learn could do so readily by reading Dunlap's little book. Neither did he intend to decry the freudians. However, Dr. Collins made the dogmatic statement: the goal of psychanalysis is to expunge the moral faculty in man; its acceptation, as propounded by its founder means abandonment of the ethics and teachings of Christianity, and any one who did not see this was mentally blind or morally oblique.

To the qualifications that the psychanalyst should have, enumerated by Dr. MacCurdy—that he should have both a medical and a psychiatric education—Dr. Collins stated that he also should have an established reputation for honesty and integrity. He should neither have it in his past that he has had to flee his native country for infraction of its laws, nor should he give six weeks' courses of instruction which purport to make psychanalysts of laymen, often lewd laymen.

Dr. Collins was anxious to hear what the members of the American Neurological Association, whose observations and conclusions he had learned to trust, had to say about psychanalysis as a therapeutic agent based either on their experience or on their examinations of its doctrines with their trained minds. He knew what the professed psychanalyst thought about it, but wanted to hear the views of the man who was entitled to be heard. Dr. Collins thought it was a diagnostic or investigative measure of great value, but a therapeutic measure of mean value. It has, however, been a good thing for medicine just as every other recrudescence of neoplatonism has been. Would any one deny today that medicine has not profited by Hahnemannism, or that Christian science had not given a great boost to neurology and psychiatry? Those stately edifices are no more firmly foundationed in mysticism than the freudian teachings.

Psychanalysis as a pedagogic measure, as a method of investigation, as a penetrating illuminating trend in psychologic research is one thing: to be investigated, to be carefully considered and evaluated and to be utilized. The

origin of the neuroses and the majority of the psychoses in the incest complex is another thing: to be denied, to be despised and to be treated with contempt. If the cause of the neuropsychoses is in some unadmitted sex desire, the way to cure it is to remove the cause, that is, to admit the perversion and gratify it. The freudians dare not say this now, but a few years hence they will say it unless God in his infinite wisdom will send us a new form of mysticism, and we are ready for one as heretofore the visitations have occurred in waves about thirty years apart. When it comes, our appetites for the supernatural will be temporarily satisfied by this more alluring pabulum.

Dr. Collins echoed the sentiment of those who disparaged the discussion of this topic from the point of view of levity, from the point of view of ridicule or from the point of view of pornography. He stated that for twenty-five years he had listened to Dr. Mills describe, narrate and discuss papers before the Society but he had never listened to him with greater admiration. He had never been more willing to call him master than after he had listened to his paper. He had shown a grasp of the subject from a theoretic point of view and had put his fingers on the weak spots of the doctrine and of the principles and of the argument, and in a measure of its application, as few men are capable of doing.

In conclusion Dr. Collins stated that no one could injure his amour propre by calling him a psychanalyst, but he did not think to add to his stature by calling himself one.

Dr. Adolf Meyer of Baltimore did not believe that the discussion had added materially to the possibility of clearing up the psychanalytic question—about as little as the interesting book of Mark Twain's had settled the problem of health cults, although it was an interesting and fascinating product of a man whom we honor, respect and admire very much. He did not think that Mark Twain's production went very far in making unnecessary the spreading of the Christian science movement and the so-called new thought idea. He felt much the same way about Dr. Mills' remarkable presentation.

Dr. Meyer said the only way really to approach this problem was to see that we actually turned to that with which psychanalysis dealt—a serious study of dynamic psychology, with actual work rather than production of literature. Up to this date there were in this country no organizations which had given an unequivocal practical support to honest work on dynamic psychobiology; hence, the principal exploitation came through uncritical and flashy literature and wholesale rehearsals in colleges to interest the instructor instead of getting students to do actual work. He thought that as long as the American Medical Association published editorials with so little recognition of the efforts of those who wanted to do serious work in the study of personality and life problems, as long as the financial foundations, which seemed to be the only sources from which money could be obtained for medical research, were adverse even to sincere and critical attacks on this field, just so long would there be trouble from laymen's exploitation of psychanalysis.

Dr. Mills mentioned the "Oedipus complex." Dr. Meyer said that he had a rather strong feeling against the promiscuous use of that term, but his feelings were softened rather than hardened when he saw it misinterpreted in this sense. Freud found himself stirred to describe a life situation in that way, a life situation which makes an impression on people because it touches certain tendencies and fears which no other description would adequately describe. He used the Oedipus myth and its meaning to the Greek world as an illustration of the potency of incest fears. Dr. Meyer said that this was true of

many psychanalytic terms. Expressions are used which are perhaps bungling and open to misinterpretations, but at least they bring out new facts as to the native tendencies of man. We could not reduce all these concepts to the principles of reflex-physiology of a necessarily narrow and static type. Neither the American Neurological Association nor any other association should close the door to honest attempts to find new terms for new facts. The chief aim should be to see that there were means furnished to study those facts, and that there should be more work, rather than literature and scintilating talk.

Dr. Charles K. Mills,[1] of Philadelphia, in replying to those who discussed and criticized his paper, said that it first must be emphasized that in a presentation of a subject like psychanalysis to a medical association, time did not permit of its thorough and elaborate consideration.

It has been asserted that neurologists, on the one hand, and academic psychologists, on the other, are not fitted to pass judgment on psychanalysis, but if those who are in favor of psychanalysis and who practice it professionally are the only ones who have a right to speak on the subject, its discussion would be altogether one sided and would not tend to the advancement of the truth. A correct judgment may be reached by a careful study of the literature of psychanalysis, by a consideration of what appears to be its fundamental principles and its methods of practice, and by observation of its effects as observed in the profession and the community.

With regard to Dr. Mills' personal experience, he said that the forms of nervous and mental diseases which have received most attention from the psychanalysts have been for many years under his observation and care both in private and hospital practice. All successful neurologists and alienists have been in a broad sense psychanalysts: Charcot, Gowers, Mitchell, Seguin, Janet, Prince and many others who might readily be named have been among those who successfully practiced psychanalysis and psychotherapy in their own way. The results obtained by these men were as good as those which are claimed by the psychanalysts of the present day.

A proper attention to sexual etiology was not overlooked by the men of this class. Every neuropsychiatrist has had much to do with certain varieties of sexual neurasthenia, or sexual anxiety neuroses, the two designations covering much the same class of cases although a separation between them might in some instances be made. In former days as in the present, many of the cases chiefly exploited in freudian literature were improved or were cured, in so far as such cure was possible, by well known physical and nonfreudian methods.

Dr. Mills went early into the field of hypnotism stimulated largely by a knowledge of the work done by Charcot, and Richer at the Salpétrière, and his experiences in this field should he believed entitle his views to some consideration. Hypnotism and psychanalysis have not a little in common, a fact which has been clearly recognized by such analysts as Ferenczi and even by Freud himself, although the latter discarded hypnotic procedures entirely as he advanced in his study of psychanalysis.

Dr. Prince, Dr. MacCurdy and Dr. Macfie Campbell lay stress—too much stress Dr. Mills thought—on what they asserted was his too great use of

1. At the meeting of the Neurological Association at Atlantic City the time was exhausted before Dr. Mills had the opportunity of replying to those who discussed his paper. The manuscript of these members of the Society has been submitted to him and his remarks in closing the subject are in the main based on this manuscript.

ridicule in attacking freudian psychanalysis. He would simply say that it was not his intention to resort largely to this weapon, sometimes allowable, in pronouncing his views on the subject. It was not to be put entirely to Dr. Mills' account if some of his auditors in some places chose to see a humorous side to the subject, or if you chose, of his manner of presenting it. If not to freudian psychiatrists, at least to many thinking men, some of the interpretations of dreams and direct analysis to be found in psychanalytic literature could not but make the recorders of the data appear to be too credulous and lacking in scientific acumen.

Is it to be held against him if others like himself found something foolish or absurd in the alleged symbolism of the "tunnel window" incident? Could he be seriously criticized because of his allusion to Michael Angelo and others who have been, as he believed, misrepresented by legends to their artistic productions? The manner in which Dr. Mills had spoken of ancient sophists comparing their terminology with that of modern freudian sophists was not, he believed, unjustifiable.

Dr. Mills said that the object of his paper was not simply to ridicule psychanalysis, but rather to give in a series of briefly presented propositions his main reasons for disbelief in recent psychanalysis and especially freudianism. It might be well for him to recall categorically although briefly these propositions. In the first place, he spoke of the tendency to mysticism or semimysticism as evidenced by different psychanalysts and, curiously enough, as regarded by different members of the psychanalytic school in their comments on each other. He also spoke of their undue use of terminology, a subject which has attracted the attention of others both abroad and in this country. The differences among psychanalysts in their interpretation of the unconscious, their incorrect methods of making use of dream analysis, and their unwarranted employment of sexual or phallic symbolisms in both dream and direct analysis were emphasized by him. In pointing out the possible benefits to be derived from psychanalysis, Dr. Mills indicated, although very briefly, the fact that freudian psychanalytic literature had done much to turn the attention of the profession to the importance of the sexual factors in the investigation of psychoneurotics. In speaking of the evils of psychanalysis, Dr. Mills called attention to the general influence of the propaganda on the moral tone or fiber of the profession and the community, and he also spoke of the particular evils which might result from the so-called process of transference, a point to which his critics had given but little attention. With regard to the subject of psychanalysis and the experiences of the recent great war, he was largely in accord with his critics and others although he did not think that psychanalytic writers had given sufficient consideration to the question of the influence of organic factors in the production of war neuroses and psychoses.

The word "dynamic" had been used in the discussion and in psychanalytic literature in a sense which was at times misleading. Every neuropsychiatrist recognizes a dynamic psychology and a dynamic pathology, but the psychanalysts of various schools at times use this term in a way that tends to the inference that there is a dynamism which has no structural machinery back of it. Dr. Prince, in private conversations and to some extent in public discussion, has shown what seems to Dr. Mills to be a decided tendency to disregard organic neurology, as for instance in his impatience to give it any consideration when describing such subjects as hallucinations.

Dr. Prince accords high praise to Freud for the doctrine of repression and conflict, but, after all, is this doctrine of repression and conflict—of course, not now using these terms simply in a freudian or sexual sense—any more due to Freud than is the doctrine of the unconscious which Prince properly refers to a group of philosophers some of whom long preceded Freud? The idea of repression and conflict permeates psychologic medicine and is exemplified in much that has been written about such a simple subject as worry, which for generations has been so frequently presented as an important etiologic factor in psychoses and psychoneuroses.

With the remarks of Dr. Schwab and Dr. McCarthy Dr. Mills was largely in accord. What had been said by Dr. Schwab regarding his experiences in France during the war coincided with the views of Dr. Mills as expressed in the body of his paper. Dr. McCarthy was a little more generous than Dr. Schwab to recent psychanalysis in the enumeration of his war experiences. Here and there Dr. McCarthy's remarks were somewhat delphic, not clearly favoring either the psychanalysts or their opponents.

Both Dr. McCarthy and Dr. Schwab indicated that in their war psychotherapy they used the older so-called "Mitchell method" quite as much as those of psychanalysis.

With regard to the remarks of Dr. MacCurdy, Dr. Mills had little to say in disagreement. Dr. MacCurdy seemed to have recognized what Dr. Mills believed he had recognized, namely, that the sexual aspects of psychanalysis had proved a dangerous lure to many in the community. Dr. Mills believed that three requirements given by Dr. MacCurdy for successful practice of psychanalysis were those which would be requisite for any method of treatment requiring intelligence on the part of the patient.

As to psychanalysis passing into the discard, Dr. Mills thought that this was true with regard to freudian psychanalysis although it was quite possible that it might to some extent tend to recrudesce as spiritualism, as all other false medical cults had a tendency at times to show their heads again.

With the remarks of Dr. Kirby, Dr. Mills said he would considerably but by no means fully agree. Dr. Kirby intimated that the freudian standpoint and the theory of pansexualism had been largely abandoned. While this might be true, Dr. Mills felt that so far as his study even of the recent literature of the subject had gone, sexual theories, largely freudian, still held a prominent place in psychanalysis. Dr. Mills said he did not attend the recent meeting of the American Psycho-Pathological Association, but was glad to hear from the speaker, and also from Dr. Prince and others that the freudian shackles were being loosened.

With regard to the psychobiologic approach to questions like the nature of the psychoneuroses and neuroses, Dr. Mills said he was certainly in accord as to its value, but simply wished to insist that this psychobiologic approach should not be restricted too much to purely sexual channels.

Time had not permitted him to discuss the question of insanity in its relation to psychanalysis. He believed, however, that the general statement of the preliminary analysis was correct, namely, that psychanalysis had not proved a success in the therapy of mental cases.

Dr. Mills said that he disagreed with Dr. Kirby when the latter said that there was nothing of the mystic element in psychanalysis. Certainly there should be nothing of this kind in genuine healthful psychanalysis, but Dr. Mills believed that he had made clear his point that any thoughtful student of the well-known literature of psychanalysis could not help see in it a ten-

dency to mysticism and he would like again to call attention to what he referred to in the paper as presented, namely, that some of the strongest protagonists of psychanalysis had discovered or believed they had discovered a mystic or semimystic element in the work of their confreres of some other subdivision of the general psychanalytic school.

With reference to the remarks of Dr. C. Macfie Campbell, Dr. Mills said that he undoubtedly in his practice tried to discover the determinists of the symptoms which were presented by his patient. Every practitioner of neuropsychiatry of any force or standing does this, but he does not and should not lay all or even the greatest stress on the sexual history of his patient unless it became clear in his investigations that it was this which played the chief rôle. As in the reference made by Dr. Campbell to the father of Darwin, it is certainly desirable in studying the affections of nervous women and everybody else to get a sympathetic insight into the causes of their nervousness, but does this mean freudian psychanalysis? One of Dr. Mills' objections was to the prolonged probing to which patients were often subjected, if he was to judge from the literature of psychanalysis and from conversations both with psychanalysts and their patients. He hardly thought it necessary to take weeks, months and even years to get a sympathetic insight into the nature of a case. He was sure in some instances of which he had knowledge that this prolonged probing had done more harm than good. Dr. Mills thought that Dr. Campbell had misunderstood his allusions to heredity. It was in connection with the views of Jung as emphasized by Nicoll that he had spoken of this subject. These gentlemen seemed to build up out of the unconscious of many generations a pet theory of the unconscious. One does not inherit the unconscious, what one does inherit is traits, trends and tendencies.

Dr. Mills agreed largely with all that Dr. Collins had said except of course his too complimentary remarks about himself. He thought he was right about neoplatonism, which was in a way a corroboration of the views expressed in his paper. He believed also that the too ardent pursuit of sexual psychanalysis certainly tended to undermine ethics and perhaps Christianity.

If Dr. Meyer in his reference to Mark Twain had Dr. Mills in mind he would only say that he thought that Dr. Meyer had been a little hard on this distinguished humorist with whom Dr. Mills said he had no right to be classed.

THE ANATOMIC SEAT OF THE EMOTIONS: A DISCUSSION OF THE JAMES-LANGE THEORY*

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It is now thirty years since Prof. William James and Professor Lange independently stated their theory of the mechanism of the emotions. This theory has never been satisfactorily proved or convincingly refuted.

JAMES-LANGE THEORY OF EMOTION

The theory, as stated by James is, briefly, that the mental state experienced in emotions of fright, anger, joy, etc., is due to different sensory impulses sent up from the muscles, viscera, blood vessels and the periphery in general. It is the palpitations, tremors, abdominal qualms and other peripheral disturbances which cause the emotion. "One is afraid because he runs; he doesn't run because he is afraid." "One is pleasant because he looks pleasant." These are rather exaggerated illustrations. Attempts have been made to strengthen this phase of the theory by obtaining the opinions of great actors as to their real emotional state when they were expressing emotion. The inquiry did not help the theory, for the experiences were conflicting.

It may be interesting to return to the neurologic quest for which James first appealed, and to see whether later experience throws any light on the subject.

Professor James thought that if he could find a case in which a person was anesthetic both without and within, and if such a person felt no emotion, it would support his theory. He did find three cases of this kind, but they were not adequately reported and not considered evidential. The subject appears never to have interested neurologists very much. Of late, however, such interest has been aroused because of the emphasis that is being placed on emotion as a pathogenic agent, and also as a kind of measure of cultural progress, for it has been noted that where emotion dominates, civilization halts or disappears. With this increased attention to emotional factors, there has come a leaning to the view that the James-Lange theory or some modification of it is true. Some seem to think that it is the perturbations of

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function in the splanchnic or vegetative nervous system that especially arouse the mental experiences in emotion. Others suggest that in addition the endocrine glands and the hormones are important peripheral agents in arousing emotional states.

On the other hand, Sherington reports one experiment, on a dog, in which he severed the cervical cord and the vagi, and found that he could still arouse evidence of emotion in the animal. This, however, is only one case and is not convincing.

The possibility of finding such cases of anesthesia as James was looking for is very remote. They could only be aroused by an extensive double lesion of the thalami. I have seen a case of complete one-sided anesthesia due to such a lesion, but with no special emotional disturbance. Sometimes a thalamic lesion causes paralysis of emotional facial expression, without particularly disturbing the emotions. One can fairly assume that if there were a double thalamic lesion causing such paralysis, the patient would feel the emotion associated with laughter or tears, without any peripheral muscular movements, and consequently no afferent reaction from the somatic system, at least. Such a patient would feel joy or fright, but could not express it in any of the ordinary muscular movements. In so far as they go, these facts tend to disprove the James-Lange theory.

If an object suddenly arouses an emotion of fear, for example, efferent impulses must be sent to the periphery before afferent impulses can be sent back. In neurologic experience we do see cases in which this efferent part of the reflex process and sometimes both pathways are very largely blocked. It is cases of this kind that I wish to report and to interpret.

Before going further let me state what meaning is attached here to the technical terms used in discussing our subject.[1] The characteristics of emotion, as described by James, in 1890, fit very well into the description of today. We say, as James virtually said, that an "affect" of "feeling tone" is something connected with sensation and has a peripheral origin. The appetites, desires, the sense of ill-being or well-being, hunger and fatigue are associated with one or more of

1. The brain-stem to which I refer later is generally described (Villiger) as including the medulla, pons and corpora quadrigemina. Tilney in his "Form and Function of the Nervous System" includes the optic thalamus. There is a difference among writers in the terms used in describing the sympathetic nervous system. The more common nomenclature is that which calls the "sympathetic nervous system" that part which is connected with the dorsolumbar portion of the spinal cord. The autonomic or "parasympathetic nervous system" is that which comes from the midbrain, medulla (vagus) and sacral part of the spinal cord.

our now estimated twenty-three special and general sensations. They are "feelings" or "affects."

An emotion, however, is always connected with a percept, idea or concept which arouses one of our primitive instincts, these being mainly variants of fear or love.[2] There is, said James, always a bodily sensation with emotion and generally some motor bodily reaction which has about the same pattern for the different emotions; that is, all persons react in about the same way motorially to fear or to joy or to anger. James emphasizes the fact that with emotion, at least with all the primal emotions, a person has some bodily sensation, such as a thrill, tremor, palpitations, abdominal qualm, faintness, etc.

EFFECT OF SOMATIC OR SKELETAL MUSCLES AND SYMPATHETIC SYSTEM ON EMOTIONAL STATES

Returning now to the James theory, I wish to present certain clinical facts, which seem to me to show that the somatic or skeletal muscles and sympathetic system proper have at least nothing but a minor and contributing effect in arousing the conscious emotional states.

I had a patient, a very intelligent woman of 40, who fell from a horse and broke her neck at the third and fourth cervical level. She was completely quadraplegic and suffered complete loss of cutaneous and deep sensation from the neck down—with abolition of all deep reflexes. A roentgenogram showed the presence of the fracture.

She lived for nearly a year, and during that time I saw her showing emotions of grief, joy, displeasure and affection. There was no change in her personality or character. The only skeletal muscles at her command were the cranial, the upper cervical and the diaphragm. As for the vegetative nervous system, she had the vagus, but the vagus has no active (low-level) sensory fibers except the laryngeal. The parasympathetic system in this patient was able to function, but the sympathetic proper was absolutely eliminated from subconsciousness and consciousness. It is difficult to understand, on the peripheral theory, why there should have been no change in her emotionally, with the skeletal system practically eliminated and the sympathetic entirely so.

Such a case as this is not so uncommon; I have seen others with similar symptoms, and they are very familiar to neurologists, but the reported case was unique in that the patient lived so long in such a normal mental condition.

Turning to other neurologic fields, we have all seen patients in the terminal stage of tabes, with perfectly atonic, anesthesic and greatly wasted skeletal muscles. In my experience their emotional reactions were present and not abnormal.

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2. McDougal enumerates six instincts with associated emotional states.

I had once in the hospital a patient with family periodic paralysis, who for a few days was completely paralyzed except for the muscles of mastication, deglutition and respiration, but with only the natural emotional and mental changes.

In the finally complete bodily immobility and paralysis of certain forms of progressive muscular atrophy, in the absolute bodily rigidity of terminal arthritis deformans, and in the advanced stages of paralysis agitans with rigidity, all muscular movements are practically eliminated, except those moving automatically and those under the influence of the vegetative nervous system. It is at least my experience that in these patients the emotional reactions are present and normal to their condition. In all these cases the vagus system is able to act, but not the somatic, and in some cases not the sympathetic.

It does not always follow that because a sensation is felt in the periphery it is due to peripheral irritation. We can have pains in the face or arm from lesions of the thalamus, and we can have thalamic paresthesia and dysesthesia. This is shown in lethargic encephalitis and in thalamic and midbrain diseases. The dysesthesias of Parkinson's disease may be in part cerebral. In fine, we know that some of the bodily sensations, such as are sometimes associated with emotion, especially anxiety states, may be central and thalamic; so that the fact that with emotion bodily sensations are felt does not prove that they come from the periphery.

Expressions of emotion, in the form of tears and laughter, occur automatically in lesions of the brain-stem and thalamus. These outbursts are not usually associated with any real emotion of sorrow or joy; and an argument based on these cases can be made both for and against the view of a peripheral source of emotion. Thus, if the source of the emotion is peripheral, why does not the man with a thalamic lesion who bursts into laughter or tears, have eventually a real emotion of joy or sorrow, since all the somatic and splanchnic changes that accompany emotion are present? On the other hand, if emotion is of central origin, why does not the person with a thalamic lesion have emotional states as a result? I would answer in the latter case that it is because in thalamic lesions there is a rupture of the relations between the thalamus and the cortex, for there must be a corticothalamic and thalamocortical activity in the emotional experiences.

A person may have a palpitation of the heart or a sense of faintness or tremor or sweating without experiencing any emotion of fear or apprehension, unless finally the idea that he is in physical danger comes to the mind. This fact is explained by various psychologic mechanisms. It seems most reasonable to suppose that when a palpitation is felt the emotion of fear develops, because an association process

arouses an idea of illness, and this causes an emotion of fear. The mechanism of an "associated conditional reflex" may also be effectively invoked in explanation.

Finally, neurologic experience also throws doubt on a certain psychologic view, lying close to the James-Lange theory, to the effect that thought and emotion are always at once reflected out upon the muscles which in turn react and return afferent impulses to the brain. The statement is made that "we think with the muscles" because every thought is accompanied by muscular movement and every muscular movement arouses some "feeling." The muscles of the larynx and articulation are assumed to do this in particular. Even in abstract thought or meditation some slight muscular action is assumed to occur in the fine muscles of the larynx.

The neurologic experience is, however, that a person continues to feel and think perfectly well after the larynx has been removed or paralyzed and also when there is a complete muscular paralysis.

AUTHOR'S THEORY OF EMOTION

On the whole, I am led to the conclusion that emotion is centrally located and results from the action and interaction of the cortex and thalamus. The bodily sensations which accompany emotion are produced by stimuli from the automatic centers in the brain-stem (acting on heart, blood vessels and glands), but they only cooperate to extend and perhaps intensify the emotion. For naturally, if a man finds his heart palpitating, his hands cold and wet, his legs weak and his epigastrium calling out in distress, he will feel more alarm. On the other hand, if the automatic stimulus produced by joy at the sight of the beloved, strengthens his heart beat, raises his blood pressure and in general improves his sense of well-being, he will feel a little happier for it.

The centers that control the activities of the vegetative system are mainly in the brain-stem.[3] These centers are first appealed to when the animal perceives something calling for instant action in defense, offense or active desire. They, on the other hand, stir up the muscles, viscera and glands, but they also appeal through the thalamus to the cortex and arouse the emotions appropriate to the object recognized or idea that has been aroused. As the suprasegmented nervous system develops, emotion develops.

3. In a recent address before the New York Neurological Society Prof. Walter B. Cannon presented evidence to show that the center for sympathetic discharges occurring on pain-stimulation lies in the brain-stem as high up as the anterior corpora quadrigemina.

I am inclined to the opinion, then, that the James-Lange theory of emotion is true only in part; that is, that the peripheral visceral stimuli are only later and contributing factors to emotion, and that the skeletal muscles and sympathetic system do not play an essential part.

I am not presenting all the data possible bearing on this question. The experiments and operations on the sympathetic ganglions, for example, show that the cerebrum can get along quite well without them or without there being much connection between these ganglions and the central nervous system. Arguments against the peripheral theory also have been presented based on studies and experiments with the glands of internal secretism. I do not attempt to introduce these data which will be found in the literature of endocrinology.

My object has been to present some facts with deductions therefrom and to elicit further contributions from the experience of neurologists.

FRONTAL LOBE PHENOMENA

AS OBSERVED IN A CYST IN THE LEFT FRONTAL LOBE; MARKED REMISSION OF SYMPTOMS; DECOMPRESSION AFTER THIRTEEN MONTHS; NECROPSY

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The clinical phenomena associated with lesions of the frontal lobes of the brain have shown a greater diversity in character and degree than those of any other locality. Not infrequently, they have been so negligible as to have left this region unsuspected during life. With more or less hesitation, neurologic clinicians have ventured syndromes as characteristic which have varied between wide limits.

Tumors of the frontal lobe have nevertheless been encountered quite frequently, as may be inferred from the series collected by Clarke,[1] in which there were fourteen instances in ninety-six cases of brain tumor.

Oppenheim[2] has said, "Apart from aphasia we know of no characteristic localizing symptom of tumor of the frontal lobe."

The difficulty of localizing by clinical signs is unusually but significantly expressed by T. Grainger Stewart[3] when he says, "The clinical picture of frontal tumor is characterized by *the absence* of signs pointing to involvement of other regions of the brain rather than by the presence of special focal symptoms." According to the same author, the general symptoms are usually present, namely, headache, optic neuritis and vomiting.

As a result of their experience with war injuries, Marie and Béhague[4] have concluded, after many observations, that deep lesions of the prefrontal region give rise to a syndrome characterized by disturbance in orientation in space, while there is an absence of all objective signs of a lesion of the nervous or vestibular systems, and headache, vertigo and dizziness were entirely absent. In the course

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1. Clarke, Frederick B.: A Study of the Anatomical Location and Histopathology of Ninety-Nine Brain Tumors, Rev. Neurol. & Psychiat. **14**: No. 11; abstr. J. Nerv. & Ment. Dis. **48**:498-500, 1918.

2. Oppenheim: Textbook of Nervous Diseases, Trans. by A. Bruce, 1911, p. 900.

3. Stewart, T. G.: The Diagnosis and Localization of Tumors of the Frontal Regions of the Brain, Lancet **2**:1209-1211, 1906.

4. Marie, Pierre et Béhague, P.: Sydrome de désorientation dans l'espace, consécutif aux plaies profondes du lobe frontal, Rev. Neurol. **35**:3-14, 1919.

of the same report, however, it is admitted that "in spite of controversy, the question of the physiological rôle of the anterior part of the frontal lobe remains obscure." Chatelain is quoted: "Up to the present time there is no true symptomatology of the frontal lobe."

Among the symptoms mentioned during a recent discussion of the subject in Leipsic,[5] ataxia of the trunk, akinesia, drowsiness, lessened activity, rigidity and fixed positions, were especially emphasized.

The frequency of muscular movements resembling those of paralysis agitans was brought out—the general impression being that these as well as ataxic symptoms were to be attributed to implication of the fronto-cerebello-pontile tracts. Likewise, symptoms referable to the posterior fossa have been frequently observed, in many of which the frontal lobe was not suspected until the necropsy.[6]

Similarly, one of the patients in the three cases of Dercum[7] was operated on subtentorially, because of cerebello-pontile symptoms, but no tumor was found; the postmortem examination revealed a tumor in the left prefrontal region. A second patient presented the characteristic described by Marie[4] in that he became lost in his own house; a tumor in the right frontal lobe was found at necropsy. The third tumor was correctly localized and removed from the right frontal lobe. In two of the cases an optic neuritis was present.

Epileptic seizures were frequently observed in nineteen of twenty-two of Stewart's series, who also quotes Müller as finding convulsions in one-third of his 168 cases. Such attacks have generally been regarded as indication of pressure on the anterior central convolution posteriorly. However, as was the experience of Foerster,[8] it was only by the revelation of the roentgenogram in a case with convulsions that the frontal lobe was incriminated and a psammoma successfully removed.

McConnell's[9] patient who had a tumor involving the first and second convolutions of the frontal lobe presented disturbance in writing when he made a voluntary attempt or when he wrote spontaneously, as well

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5. Jahres Versamml. d. ges. Nervenärzte, Neurol. Centralbl. **19**:649, 1920.

6. Ruckert, A.: Ein Stirnhirntumor unter den klinischen Bild eines Tumors der hinteren Schädelgrube, Berl. klin. Wchnschr. **46**:1248-1251, 1919. Souques, A.: Des Troubles Auditifs dans les Tumeurs Cerebrales, Rev. Neurol. **12**:727-731, 1904.

7. Dercum, F. X.: A Report of Three Prefrontal Tumors, J. Nerv. & Ment. Dis. **37**:465-480, 1910.

8. Foerster, O.: Discussion: Zur Diagnose von Stirnhirntumoren (Bostroem, A.), Neurol. Centralbl. **19**:650, 1920.

9. McConnell, J. W.: A Case of Tumor of the Left First and Second Frontal Convolutions with Motor Agraphia as Its Chief Localizing Symptoms; Successful Removal of the Tumor, Univ. of Pa. Med. Bull. **18**:1905; abstr. J. Nerv. & Ment. Dis. **33**:211, 1906.

as when he wrote from dictation, while when copying his writing was "practically perfect." His speech was thick which "appeared to be due to paresis of the muscles of articulation, enunciation and phonation." Mention is made of omissions, faulty spelling and formation of words in a letter written two years before observation. Memory and attention had failed. There were convulsions and hemiparesis of the face.

A tumor was removed by operation which was lying across the foot of the second frontal convolution, encroaching on the lower half of the first, slightly on the posterior portion of the third and the anterior edge of the precentral convolutions.

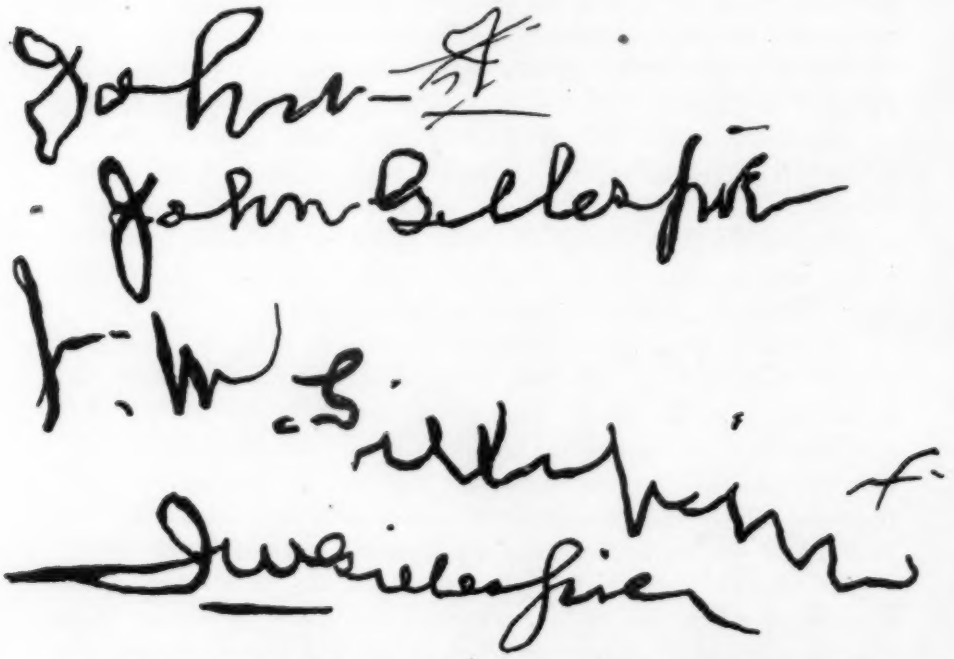

Fig. 1.—The various changes in handwriting as improvement progressed.

Unilateral anosmia and the occurrence of optic atrophy on one side with more or less neuritis on the other have been cited, but obviously could only apply where the location closely approximated the inferior surface of the frontal lobe.

Psychic symptoms are not infrequent, according to Stewart, but are probably greatly exaggerated. These concern chiefly disturbance in attention, delayed response or other evidence of slowness in thinking; various degrees of loss of memory; some irrelevancy with jocularity; apraxia in general and as related to speech; the disturbance in orientation, especially in direction or space, rather than with reference to

time (Marie);[4] and as regards cerebral activity, from all degrees of coma and stupor to mania with active delusions.

In arriving at a diagnosis of frontal lobe tumor, Grainger Stewart[8] says that we have to rely on (*a*) the presence of the general symptoms of an intracranial growth, (*b*) the presence of mental symptoms, and (*c*) the absence of focal signs pertaining to other regions of the brain.

The case which I report presents not only a verified lesion of the frontal lobe without general symptoms and with an absence of the frontal lobe symptoms mentioned, but also includes speech and graphic disturbances with a predominance of apraxia as related to these functions as well as to movements with less highly developed purpose; and these were sufficiently distinct to direct the attention to the left inferior frontal lobe. These manifestations and the intellectual state are more interesting because the patient was a practicing attorney of prominence and reputation, whose preliminary training included a university course in arts, followed by a baccalaureate in law from the University of Michigan.

REPORT OF A CASE

History.—J. W. G., aged 44 years, unmarried, attorney at law, was first observed Feb. 17, 1919, complaining of vertigo and slight edema of the hands which had been noticed during the past two or three years. His mother had died suddenly a year before (apoplexy). His father was in good health. He had no brothers or sisters. A paternal grandfather died of cancer. Otherwise his history was negative as to constitutional, nervous or mental disease. He had had measles, whooping cough and mumps in childhood. No illness other than attacks of biliousness were admitted. Gonorrhea and syphilitic infection and its manifestations were denied; there was no alcoholic excess. There had been no serious injury and he had never been unconscious from any cause. He had always been robust and athletic in development and practice.

The present complaints included vertigo and transitory edema of the hands. There was no headache, no vomiting or numbness. A zig-zag scotoma was admitted. Dizziness was increased on stooping and was characterized as "bilious." His appetite was good; he had no digestive distress. The bowels were usually regular. Sleep was good and restful.

Examination.—The patient was a man of excellent development, 5 ft. 9 in. in height, weighing 178 pounds. The heart borders and sounds were normal. Blood pressure (Faught) was: systolic 140, diastolic 85. There was no motor disturbance—voluntary, associated or reflex. All deep reflexes were present, equal and about normal in degree. Ocular movements were present in all directions without nystagmus. The pupils were equal and responsive. The fundi were normal. The urine was acid; specific gravity, 1.024; no albumin; no sugar; no bile; no casts—cylindroids, no epithelium.

Second Admission.—July 5, 1919, the patient returned complaining of numbness of the right hand—more marked in the thumb and index fingers. Difficulty had been noticed in the finer purposeful movements of the fingers, especially evident in buttoning and unbuttoning his coat. The sense of numbness included at times the whole hand and, to some extent, the arm and varied in degree and extent.

Second Examination.—Examination at this time failed to reveal disturbance in the detail movements of the fingers, and the associated movements were well executed. The tongue was protruded in the middle line, but heavily coated. The mouth was more movable on the left side than on the right.

The tissues of the right hand were softer and more flabby to palpation in contrast to the opposite side. Power in the right hand was slightly less than in the left; the dynamometer (Tiemann) registered: right, 140; left, 190. The reflexes of the right forearm were prompt. Faradic responses of the muscles of the right forearm were prompt, with a current of moderate strength. The pulse was slow, regular and of good volume. The optic fundi showed no change from previous observation. The urine was normal, except for a distinct trace of indican.

July 11, 1919: There was some relief following thorough eliminative medication, but attacks of vertigo were noticed and, on one occasion, he fell.

A disturbance in speech was experienced on two occasions—on one a difficulty of articulation and thickness of speech were recognized, and on another the inability to recall words in order to express an opinion during a conference or consultation in which complex legal points were involved.

The numbness directed to the thumb and forefinger of the right hand persisted. Examination at this time revealed no objective evidence. The brow was evenly wrinkled. The tongue was mesially protruded and was moderately coated. The mouth was retracted quite evenly. Associated and detail movements of the hands were well performed. The dynamometer registered: right, 290; left, 280. He walked well forward and backward with the eyes open or closed and maintained himself without unsteadiness under the same conditions. All of the deep reflexes were present and equal. The plantar reflexes were present and flexor in type.

Special Senses: Notwithstanding the subjective complaint of numbness, no disturbance in sense perception could be made out, especially in the radial area of the back of the right hand, which was felt as "numb and puffy." There had been and was some vertigo entirely subjective in type, that is, he felt as though he were turning in space.

Eyes: All external ocular muscular movement was present without nystagmus. The pupils were equal and responsive to light and distance. The fields of vision, as estimated by testing with colored pencils, showed slight contraction in the right, both as to form and color; accurate testing was not done at this time. The right optic fundus was congested and there was slight blurring of the margin of the nerve head; some tortuosity of the vessels was evident; the left fundus was normal, except for slight capillary injection.

Hearing, Taste and Smell: Hearing was not impaired as far as the speaking voice was concerned. Taste and smell were not impaired.

Course.—July 15, 1919: The patient had been recumbent for the greater part of the time and under iodin medication. There had been transitory attacks of paraphasia in which it was difficult to find the word desired, and at times words were misused. There was slight weakness of the right side of the face. Association as well as detail movements were well accomplished. Gait and station were not impaired. Ocular movements were not altered, and the pupils were equal and responsive.

July 16, 1919: After having been up, dressed and about the house, contrary to instructions, the patient became suddenly unable to speak in that he was unable to find words with which to express himself, and the attempt to do so

led to confusion, perplexity and gross misplacement of words. The ability to execute muscular movements as purposed failed, whether by verbal direction or by imitation. Although able to walk, there was slight unsteadiness. Detail movements of the fingers—for example approximation of the thumb with other fingers—were lost and if exemplified could not be imitated. Instead of approximating the fingers by imitation he clenched the fist; the error was not recognized.

The facial movements showed weakness on the right side without twitching and more pronounced on emotion. There was no disturbance in coordination of automatic movements, as in walking and movements of the hands; but the acts proceeding from voluntary purpose, while coordinated, were quite foreign to purpose.

Laboratory Findings: (July 18, 1919, Dr. Ward Burdick). Blood Serum: The Wassermann test revealed 100 per cent. hemolysis with two cholesterinized and three plain antigens. Cerebrospinal Fluid: Pressure was moderately increased; the fluid was clear, transparent and limpid; it contained 10 cells; colloidal gold curve, 0000011000; globulin, present (+); Wassermann reaction, 100 per cent. hemolysis with cholesterinized antigen and 2 c.c. of fluid. Morphologic Analysis of Blood: leukocytes, 10,800 (polymorphonuclear neutrophils, 75; lymphocytes, 15; large mononuclears, 9; eosinophils, 1).

July 30, 1919: Improvement, since last notation, had been chiefly in the direction of voluntary speech. The patient was able to recognize objects and their uses readily, was able to recall objects when named and to point them out at will. Considerable difficulty was encountered in computations in figures. Oral spelling of words was difficult, and frequent errors were made in small, simple words. He was able to read understandingly and to think in speech. Musical airs were remembered when mentioned and recognized when heard. Test words were repeated with fairly good articulation. In voluntary speech, however, considerable dysarthria was manifested at times over simple words; this was especially noticeable with the contained consonants, and he would frequently repeat several words rhyming with the one he desired to use, stopping promptly when the correct word was enunciated. For example, in attempting to use the word "drop," he repeated "crock," "crop," "trop," and finally stopped with the correct word "drop," recognizing it as the one desired.

All movements were well executed, except that the emotional movements of the mouth were still more pronounced on the left side. The pupils remained small, but the vision of the right eye, especially with reference to the right field, was complained of as "hazy." The optic fundi showed no greater change than slight capillary injection without interference with the nerve margin. Numbness was persistently experienced in the right hand, more especially in the radial or area of median supply. The power of the right hand was diminished; the dynamometer registered: right, 220; left, 275. There was no change in the reflex responses; the plantar and Oppenheim reflexes were frankly flexor in type.

The fields of vision, as taken by perimeter, showed slight constriction, more marked on the right; no alteration in color relations and no interlacing was observed (see chart).

The speech disturbances were carefully studied with the assistance of Dr. C. S. Bluemel, who kindly made the following observations and notes: Aug. 6, 1919: The patient greets the doctors with some pleasure and

animation and opening the door for them to enter the house, he says, "Sit down;" at the same time he nods at the chairs on the front porch with a little confusion, giving the impression that with fluent expression at his command he would have asked whether the doctors would have preferred to remain outside or go in the house.

When introduced to Dr. B. he does not readily find the conventional expressions, and he expresses the amenities more by the hand-shake, nodding and by facial expression. He shows similar paucity of verbal thought throughout the examination, and his facial expression and manner suggest that he would frequently express ideas if the words were forthcoming.

His verbal expressions are usually accurate in pronunciation, but they lack continuity or coherence, and thus his sentences or phrases seem to be hyphenated. He never indulges in any lengthy verbal expressions, but rather in short and

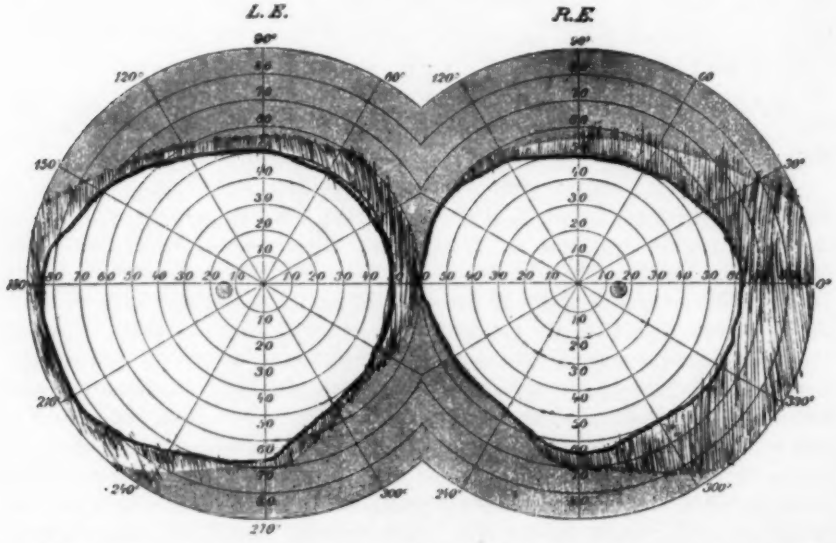

Fig. 2.—Concentric constriction of the visual fields with suggestion of right homonymous hemianopsia, Oct. 4, 1919.

isolated sentences. Occasionally his words seem apposite, being rather inappropriate synonyms for the words he cannot recall. Sometimes his sentences are unfinished owing to his inability to recall appropriate words.

His manner clearly shows that he realizes his disability and he recognizes the correct word when he finally pronounces it, or when it is supplied by another person. His chief difficulty seems to be his inability to recall the appropriate words, and he remains silent during the greater part of the conversation despite the fact that his attention is keen and his manner alert. He frequently slurs the pronunciation of words, or elides consonants or syllables. Occasionally he pronounces the first syllables correctly, then flounders with the remainder of the word or mispronounces it (dict-ionary, motor-neer for motorman).

He cannot readily recall and recite short rhymes that have been taught him. He does not do mental arithmetic well. The product of three times

forty-six is given as one hundred thirty-one; after correctly figuring aloud, "three sixes are eighteen; three fours are twelve," he appears to lose the mental image of the eight for which he substitutes a one.

There is no word deafness, and he understands all that is said to him. In general he complies exactly with any oral instructions, but has difficulty when told to place the thumb of the right hand in contact with the third finger of the right hand. Later he executes several similar tests with the thumbs and fingers of the two hands, but seems to experience a little difficulty in thinking out the instructions—he then executes them promptly.

Written instructions (to place the right hand on the left ear, pinching the chin with the left hand, etc.) are promptly executed. He read the headlines from newspapers correctly, but in a broken disconnected manner.

There is difficulty in spelling words, especially the longer ones. "European" is spelled "Euroupean" and he does not recognize the error. He also fails to recognize the error when the word is written out for him. He correctly spells such names as Holmes, Haines, Haynes, which are given him as visualizing tests. He promptly gives rhyming words (pay, sleigh, ray, etc). His manner is at times a little bizarre and extravagant.

August 8, 1919: The patient is more fluent than when seen two days ago. He engages in more protracted conversation, though with frequent delays, during which he searches for missing words, also with frequent mispronunciation. In his paraphasia he usually pronounces the initial syllable or consonant correctly. In endeavoring to say that he is having less difficulty with speech he finds himself checked at the word "difficulty;" he says, "di-dis-difficulty."

There is no evidence of dysarthria, and when he fails to pronounce a word he looks perplexed as though he had difficulty in recalling it. He says, "I can't think of what I like to think of. Then I supply. See?" When told to think of the Lord's Prayer to himself and to indicate when finished, he indicates its completion in twenty-eight seconds; thus finishing almost simultaneously with the examiner.

He cannot recall the lines taught him. "A little learning is a dangerous thing; drink deep, or taste not the Pierian Spring." He says he cannot recall the third word. The following words are written out for him and he is told that one of the words supplies the omission: "Beer, tiredness, knowledge, warfare, love." He selects the word "knowledge," but says this is not correct and brings a book in which the couplet is printed. He then demonstrated that the word is "learning." When asked to write the couplet, he does so with little hesitancy, seeming only to be puzzled about the words "thing" and "spring." He then studies the words carefully before adding the final "g".

Asked to read from the paper, he does so a little slowly and cautiously. He pronounces the word "game" as "dame" and then corrects himself. At the word "threatened" he says, "the" then reads the word correctly. The word "statements" he reads with marked pause between the syllables. "Congress" he pronounces as "college." "Protesting" he gives as "potesting," and he does not correct this error. He states that reading is difficult because of blurring of the eyes; that there is more marked blurring on the left side. Here he says "left" and confirms his meaning with a gesture of the left hand. He says it is very "tite-tiresome" for him to "go through it" (to go on reading).

When asked to recite the couplet mentioned above he begins, "A little something is a dangerous thing." "It is the third rule, road, word, that tea-teases

me." Later he says, "bring deep" for "drink deep." In a subsequent conversation he says "cattle" for "capital" (capital letter); then corrects himself. In speaking of his aphasia, he asks, "Don't you think it will wear out? I really nee- it, need it, in my procession-profession."

He whistles the tunes of "Old Kentucky Home," "A Perfect Day," and "America" correctly.

The following surnames are pronounced for him and he spells them orally thus: Fitz (giving Fitts as an alternative spelling), Phelps, Cochran, Sorrenson (also Sorrensen), Spaulding, Spencer (also Spenser), Sprague, Pratt, Prescott, Profitt. Asked to give the capitals in the following names, he spells them as follows: McCormack, MacFarland, McPherson, McCumber, McPhee and McGinnity. The name Cornelius, he spells as "Corneilus" and then adds, "I think." During the testing with these names he is given the surname "Primeaux" and told that the name is French. He spells it "Primo"; then adds, "Unless you want to take it the easy way." When asked what the easy way would be, he again spells it "Primo" (apparently there is momentary confusion).

August 11, 1919: The patient shows further improvement in speech, and at times uses rather long sentences, or several consecutive, short sentences, without error. It is noted that he flounders less with difficult words and seldom makes more than one mispronunciation of a difficult word before he correctly articulates it. He frequently pauses for an instant before a word (suggesting a temporary inability to recall it) and then pronounces it correctly. Thus he says that he is "taking it—easy," and that he is a "little bit—better."

Quite frequently he makes several false starts at a sentence, changing and rechanging the construction, in order to introduce the words that he is able to command and apparently to avoid words that are lost to him by amnesia.

Circumlocutions are quite evident in his conversation. When apparently endeavoring to express the idea that he is talking to give me an opportunity for observation, he says, "I was trying to talk about the thing to give you the range; I was trying to catch the play." When apparently endeavoring to say, "Let me see," he says, "Well, I may see."

During the conversation the following words are recorded as giving the patient difficulty. They are pronounced correctly at the second attempt; the initial attempt is given in parentheses: ride (road); county (ca—); fingers (figures); years (yeal); efforts (eck); states (stages); practically (practi—); syllable (syllas).

For circuit he says "circitit." He says "these two thumbs" for "these two fingers," then corrects himself. He says "consolidization" for "consolidation." When the word "consolidization" is pronounced for him five minutes later and he is asked to point out the error he promptly says that it should be "consolidation." He still pauses at the word "drops" and it seems probable that certain definite words cause him uniform difficulty. He states that he can speak no better when alone than when talking to other people.

He still complains of dizziness and blurring of the eyes (especially in the left visual field). There is still numbness of the index and middle fingers of the right hand, but this tends to be limited now to the tips of the fingers. No headache is admitted.

April 11, 1920: About the middle of February, 1920, he began to complain of severe neuralgic pains in the back of the neck. He is said to have been able

to talk well and to go about without evidencing impairment in voluntary movements. Because of the pain he was put to bed and perspiration induced by a hot pack. The pain suddenly lessened, but on the following morning, on attempting to speak, he was able only to mumble inarticulately. Complaint was made that vision was weak and misty, while mentally he wandered in a delirious manner; there were transitory and fragmentary delusions, such as the belief that his father had been married while absent on a visit to the city. Marked restlessness with rolling from side to side supervened. On attempting to walk he was unable to maintain himself, staggered and required to be held in order to avoid falling.

At this time he was lying in bed indicating a recognition of those who came into view. He replied responsively but articulation was so markedly impaired as to be scarcely intelligible. There was reason to believe that he understood all that was said to him, as indicated by his compliance with requests involving

Fig. 3.—Hernia of the frontal lobe following the primary decompression.

responses of both psychic and motor form. The right hand failed in associated movements, and there was definite dysmetria in either arm. On standing there was much incoordination and some propulsive effort, requiring support to avoid falling. Vertigo was not admitted; there was no headache or vomiting, and he complained of nothing. Sphincteric control was maintained. The brow was evenly wrinkled; the face drew more to the left side on emotion, volitional attempts being about equal. The tongue was protruded in the middle line without tremor or atrophy. Other voluntary movements evidenced no alteration in power but much lowering in coordination and especially in movements requiring precision. Direct percussion of the skull over the frontal areas revealed a distinct difference of the two sides in that a high-pitched note was constant on the left, while a dull, "cracked-pot" note, somewhat fluid in character, obtained on the right side.

Roentgenographic examination showed a distinct shadow in the left frontal area, extending backward to the central fissure. Roentgenograms of the teeth indicated considerable pyorrhea and peridental abscess formation.

Reflexes: All of the deep reflexes were present and about normal in degree and equal on the two sides. The superficial reflexes showed no deviation from the normal, both plantar reflexes being flexor in type and the abdominal reflexes being prompt and equal.

Eyes: All external ocular motion was prompt; convergence was well accomplished, and no nystagmus was evident. The pupils were equal and promptly responsive to light; the right was slightly irregular in contour. The optic fundi were apparently edematous, with very moderate swelling—somewhat less than on previous observation—and no distention of the capillaries.

Hearing: This sense was not impaired for the speaking voice.

April 17, 1920: A decompression operation was done by elevating an osteoplastic flap over the left frontal area; considerable pressure with distention. Insertion of a probe was followed by collapse and drainage of clear fluid. The wound was closed without drainage.

May 12, 1920: A secondary operation was performed under ether anesthesia, and the previous wound was reopened; there was considerable hernia of the brain, especially in the anterior portion of the skull opening. Resistance at a depth of one and three-fourths inches, which later proved to be the wall of a cyst, was encountered. A wick type of drain was placed and carried outward external to the pericranium, the dura was closed about the drain and the scalp brought together in the usual manner. The time required for the operation was one hour; the general condition of the patient was good. The hernia at the site of the operation recurred with considerable distention, which could now be relieved by lumbar drainage.

There followed a gradual decline, more pronounced weakness of the right side of the face, with increasing unconsciousness and death Aug. 2, 1920. There were no convulsions at any time.

Necropsy.—Three hours after death necropsy examination was performed by Dr. Ward Burdick. Permission to examine the brain only was obtained. On removing the calvarium firm adhesions were found under the site of the operation. The convolutions showed no abnormality other than slight injection of the capillaries. The meninges were clear. The right ventricle was somewhat dilated. A cystic cavity was found underlying the second and third frontal convolutions of the left side at about the junction of the middle and posterior third, measuring 4 cm. anterioposteriorly and 2 cm. at its smallest diameter. The cavity was filled with broken-down pultaceous matter and communicated with the lateral ventricle, which was somewhat distended with fluid. Small sections of what appeared to be a fairly defined limiting zone or membrane were retained for examination. Sections of the brain failed to reveal abnormality elsewhere. In this cavity communicating with the ventricle was found a silk ligature drain which was placed at the time of operation.

Pathologic Report.—(By Dr. Harry Gauss). Two pieces of tissue, about 1 cm. each, were received for examination. These consisted chiefly of gray, with some white, substance. The white substance terminated in a grayish-brown membrane-like band of about the thickness of a hair. The tissues had been hardened in liquor formaldehydi for about a week. Preparation for examination included the usual washing, dehydrating, imbedding in paraffin and staining with hematoxylin and eosin. The essential pathologic alterations consisted of miliary punctate hemorrhages, edema, round cell infiltration and neurologic proliferation, These are best described in detail under the several headings:

Hemorrhages: Small miliary punctate hemorrhages were seen in the pyramidal layers of the gray substance. These were round and sharply limited; they measured from 100 to 300 microns and were located chiefly about small blood vessels.

Edema: Throughout the cyst wall, also in that portion of the white substance lying between the wall and the gray substance, as well as in the lamina multiformis of the latter, there were numerous clear distended lymphoid spaces which were extracellular and extravascular. These were numerous and varied from 5 to 30 microns in size.

Round Cell Infiltration and Neuroglia Proliferation: The cyst wall was composed principally of infiltrating small round cells and neuroglial tissue which appeared to have undergone moderate proliferation. The infiltrating cells were almost entirely of the small lymphoid type, although a few polymorphonuclear cells were present. The zone of white cells appeared to form a definite band along the white substance and, generally, did not invade the gray substance, except in a few small areas in which it extended into the lamina multiformis.

These inflammatory reactions indicate a chronic process of benign type. Pathologic diagnosis: A serous cyst of the frontal lobe of the brain.

CONCLUSIONS

It seems obvious from the evidence furnished by the study of this and other frontal lesions that subcortical lesions of the left frontal lobe, and especially the second and third convolution, are likely to be associated with disturbance in movement as proceeding from a preconceived purpose (apraxia), when occurring in a right-handed person. It would appear that progression posteriorly might give rise to disturbance in speech or writing as purposed (apractic aphasia or agraphia) and finally dysarthria. Localized spasm followed by paresis would suggest involvement of the central convolution, which may become generalized, especially if the callosum is included. Incoordination and ataxia may be associated and attributed to mesial extension into the elements of origin of the frontocerebellar tract. (It will be observed that no spatial disorientation was found in the case presented at any time prior to supervention of coma; the knowledge of the patient's surroundings in the hospital and at home was clearly evident at all times.)

Vomiting, headache and optic neuritis may be absent in subcortical lesions of the frontal lobe, especially in the absence of encroachment on the membranes. Unilateral optic atrophy with papilledema of the contralateral nerve is to be interpreted as proceeding from the inferior surface, although bilateral papilledema may occur as a result of rapidly increasing intracranial tension induced by frontal growths as well as by those occurring elsewhere.

325 Mack Building.

A CONTRIBUTION TO THE HISTOPATHOLOGY OF PORENCEPHALUS*

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Porencephalus is always of interest, partly because of the striking disproportion between the great loss of brain substance and the mild degree of functional disturbance. Unfortunately, however, almost all studies of such brain lesions have been limited to gross anatomic observations. It is hoped that this study adds a few facts to our knowledge of the histopathology of porencephalus. Incidentally it emphasizes the close relationship of such defects to the inflammatory encephalitides.

The term porencephalus was first adopted by Heschl,[1] in 1859, to designate such defects in the brain substance as are characterized by cavity formation in the cerebral hemispheres. Such cavities, to be within the limits of his definition, must communicate either with the subarachnoid or ventricular space. Moreover, the defect might be so pronounced as to lead to intercommunication between the subarachnoid space and the ventricular cavity.

Heschl assumed that this loss of brain substance was the result of faulty development. Some injury had occurred during the intra-uterine life to the anlage of certain parts of the brain cortex, resulting in failure to develop, leading also to malformation of remote and dependent fiber tracts. He drew his conclusions from gross anatomic studies of a fairly large number of porencephalic defects.

Later [2] he admitted that porencephalus might be due to a regressive, destructive process occasioned by occlusion of cerebral vessels. He found histologic evidence of degenerative changes, which he interpreted as being vascular in origin and perhaps syphilitic in character.

Kundrat,[3] in 1882, made a further study of porencephalic brains. He reexamined all of Heschl's specimens and added about twelve of his own. His observations, limited mainly to gross anatomic studies, can be summarized as follows:

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* From the Department of Pathology, Mount Sinai Hospital, New York.
* Work carried out during the tenure of a Fellowship in neuropathology.
* Presented before the New York Neurological Society, April 5, 1921.

1. Heschl: Gehirndefect und Hydrocephalus, Prag. Vrtljschr f. pract. Heilk. **61**:1859.

2. Heschl: Neue Fälle von Porencephalie, Jubelaeums band der Vrtljhrschr. **100**:40, 1868.

3. Kundrat: Die Porencephalie, Eine anatomische Studie (monograph).

1. Most of the porencephalic defects are congenital, although infrequently they may be acquired later.

2. Hemorrhage, thrombosis, embolism, violent uterine contractions, hydrocephalus, anemia and psychic disturbances occurring to the mother during the period of pregnancy, are etiologic factors.

3. The porencephalic loss may be so pronounced that the life of the individual is early terminated, or the destructive process, extensive as

Fig. 1.—Right surface of the brain, showing thickening of the leptomeninges, atrophy of the parieto-occipital lobes, and loss of normal cerebral topography. The irregularly stippled and corrugated surface is particularly noticeable in the region of the parieto-occipital lobes.

it may be, not involving any vital centers, permits the individual to mature, although physical and not infrequently mental deficiencies may be striking.

4. Destruction or atrophy of part of a hemisphere may be compensated for by relative hypertrophy of the opposite hemisphere.

The main contribution of Kundrat was extension of the term so as to admit other cerebral malformations, which, while not presenting typical cavity formation, are nevertheless characterized by extensive loss of brain substance and by other pathologic features similar to those recognized in the typical form of Heschl. Another suggestion of doubtful value is that "anemic encaphalitis" is responsible for the defect in instances showing no evidence of trauma or changes in the

Fig. 2.—Extreme distention of the lateral ventricles and marked thinning of the cerebral hemispheres.

blood vessels. He finds an analogy in the ischemic softening of senile persons without demonstrable changes in the cerebral vessels.

Soon after Kundrat, Schultze [4] suggested that porencephalus may be due to infectious encephalitis. He criticizes Kundrat for failure

4. Schultze, F.: Beitrag zur Lehre von den Angebornen Hirndefecten (Porencephalie), 58 Versammlung Deutscher Naturforscher und Aerzte in Strassburg, 1885.

to study his material microscopically and denies the possibility of anemic encephalitis. He found microscopic evidence of inflammatory changes, probably of infectious origin. He further concluded that the destructive changes began during intra-uterine life.

That prenatal encephalitis is possible is shown by the observations of Virchow,[5] who established the existence of congenital encephalitis as the result of an infection in the mother. He says: "It is conceivable

Fig. 3.—A porencephalic defect opening into the lateral ventricle. Smaller defects are seen in the vicitnity of the porencephalic cavity. The pia-arachnoid is thickened and fibrous in structure. Photomicrograph; × 17.

that such encephalic and myelitic affections are not always fatal and that instances occur where healing takes place, although this fact is yet to be established by further research. . . . It is most probable

5. Virchow, R: Congenitale Encephalitis und Myelitis, Virchows Arch. f. path. Anat. **38**:129, 1867.

that many cases of idiopathic and deuteropathic paralyses of the infant and many instances of idiocy can be traced back to such encephalitic changes."

Strümpell [6] studied acute encephalitis in the infant, clinically and anatomically, and concluded that many porenecphalic defects are due to encephalitis.

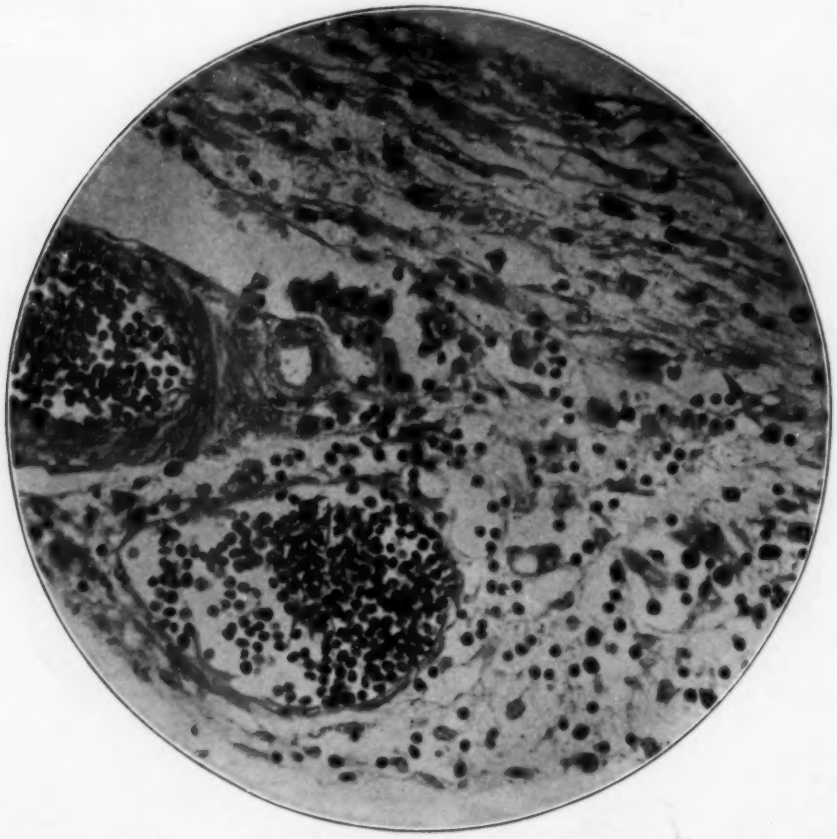

Fig. 4.—Section from the leptomeninges of the central gyrus of the frontal lobe, showing distention and infiltration of the pia-arachnoid space; vacuolated macrophages, small lymphocytes, large mononucleated leukocytes and fibroblasts. The vessels are distended and engorged with blood. The adventitia is thickened and infiltrated. Photomicrograph; × 300.

More recently Limbeck,[7] who studied four cases of porencephalus, has supported the views of Strümpell.

6. Strümpell: Ueber die Acute Encephalitis der Kinder, Jahrb. f. Kinderh., 1884, vol. 22.

7. Limbeck: Zur Kentness der Encephalitis congenita in ihrer Beziehung zur Porencephalie, Ztschr f. Heilk. **7**:1886.

Sachs and Peterson [8] say: "Porencephalus is a secondary condition and although much has been written on the subject, we know little of its origin. We are willing to concede that some of the many cases of atrophy or sclerosis may have been due to polioencephalitis, but it

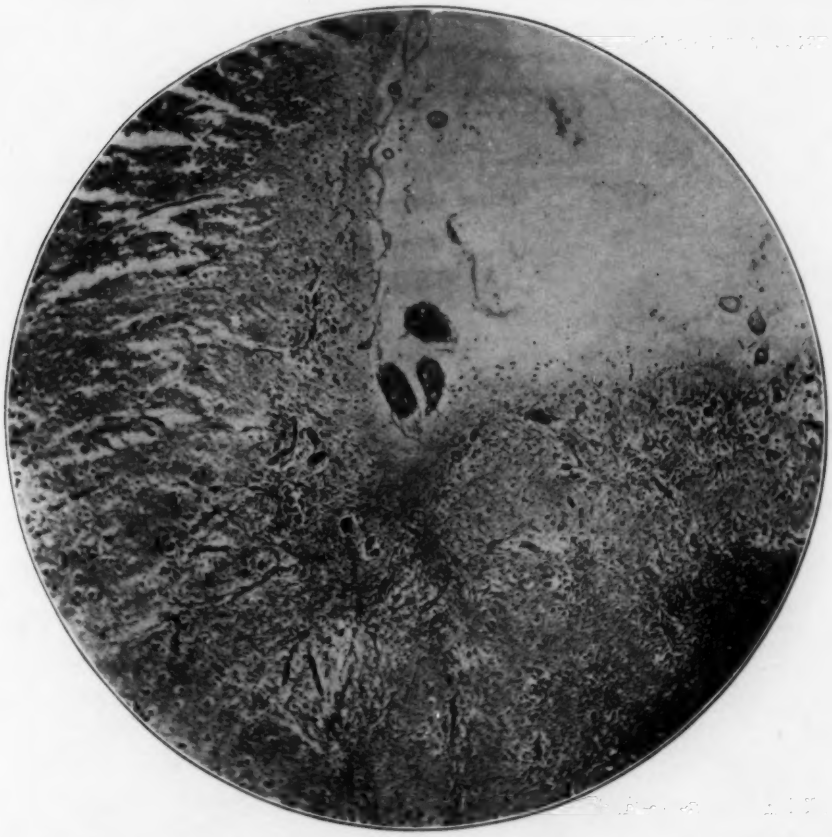

Fig. 5.—Note the marked thickening and fibrous character of the pia-arachnoid covering the parietal lobe. The underlying cerebral cortex is replaced by sclerotic patches. While the vessels on the surface are distended and engorged with blood, the vessels in the cortex are obliterated and represented by fibrous bands. Photomicrograph; × 70; Bielschowsky-Alzheimer-Mann stain.

is unfortunate for Strümpell's theory that all of the necropsies after death have shown other conditions but not encephalitis."

8. Sachs and Peterson: A Study of Cerebral Palsies of Early Life based upon an Analysis of One Hundred and Forty Cases, J. Ment. & Nerv. Dis. **17**:319, 1890.

CASE REPORT

History.—G. N., 11 months of age, the first child, was born after a full term pregnancy and normal delivery. There was no evidence of syphilis in the parents, who gave a negative blood Wassermann reaction. At birth the patient was well formed, and he developed normally until the age of 4 months when it was noted that he did not progress mentally and acted as though he were blind and deaf. Aside from a small head, the child showed no physical maldevelopment.

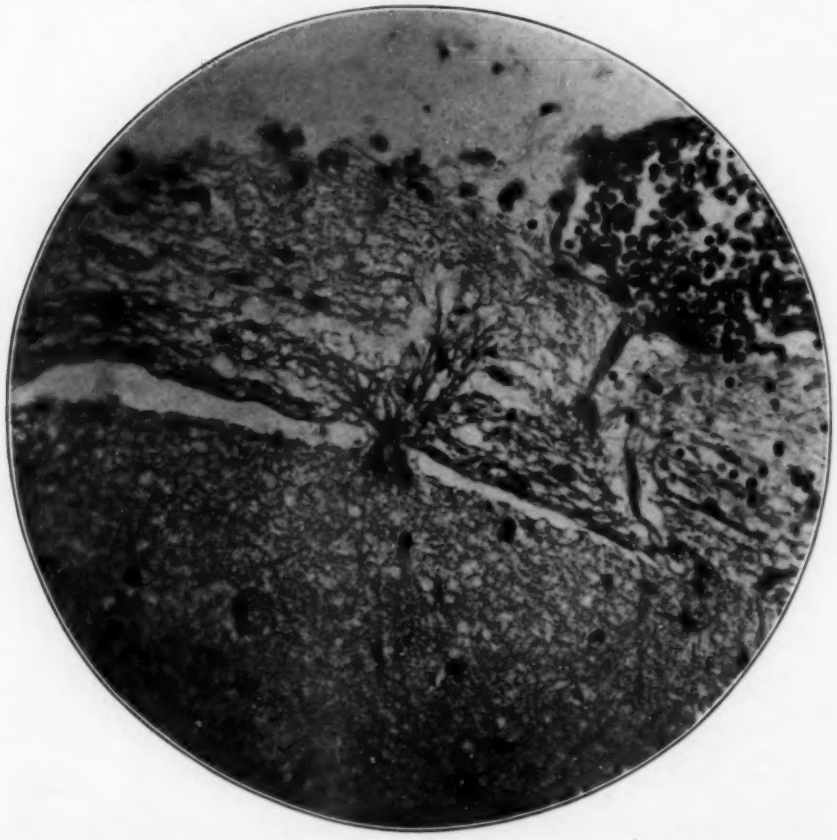

Fig. 6.—Glial cells are seen sending out their processes into the pia-arcahnoid, bridging over the sub-arachnoid space. Photomicrograph; × 300.

Examination.—On admission to Mount Sinai Hospital the following findings were recorded:

Child well nourished and well developed. Head small; fontanelles closed. Pupils regular, react to light and accommodation, no nystagnus. Upper extremities spastic and in flexion. Lower extremities spastic; all deep reflexes exaggerated. Temperature irregular ranging from 99.5 to 102 F.

Treatment and Course.—The diagnosis was congenital microcephalus, and bilateral subtemporal decompression was advised, which was performed four days later. After operation cerebrospinal fluid escaped from the wound, but the

patient's condition was unchanged. A high variable temperature continued. On the eighth day the child had several attacks of generalized convulsions, grew rapidly worse and died the same day.

Necropsy.—Heart, spleen, liver, and suprarenal glands showed no gross or microscopic lesions. The lungs, with the exception of some emphysema, were normal. The kidneys showed a mild degree of cloudy swelling.

Fig. 7.—Glia cells are seen in large numbers streaming into the pia-arachnoid alongside vessels that are still patent in their meningeal course, but hardly penetrating the cerebral cortex. Camera lucida drawing (Zeiss ocular 6 × objective 3).

The brain was rather small and apparently somewhat collapsed due to postoperative escape of cerebrospinal fluid. The meninges over the dorsolateral surfaces of the cerebral hemispheres were thickened in some areas and firmly adherent to the underlying cortex; in other areas they were greatly distended by fluid. The meninges at the base were normal.

On palpation the hemispheres fluctuated due to marked increase of cerebrospinal fluid in the ventricles, and particularly because of thinness of the walls. Right Hemisphere: The sylvian fissure was well developed at the base but rather wide and shallow in its posterior part. It lodged the middle cerebral artery which ran its normal course over the frontal lobe and for some extent over the parietal. The frontal and parietal opercula were poorly developed, exposing the island of Reil, while the temporal operculum was normal. The convolutions of the frontal lobe could be made out although the fissures were

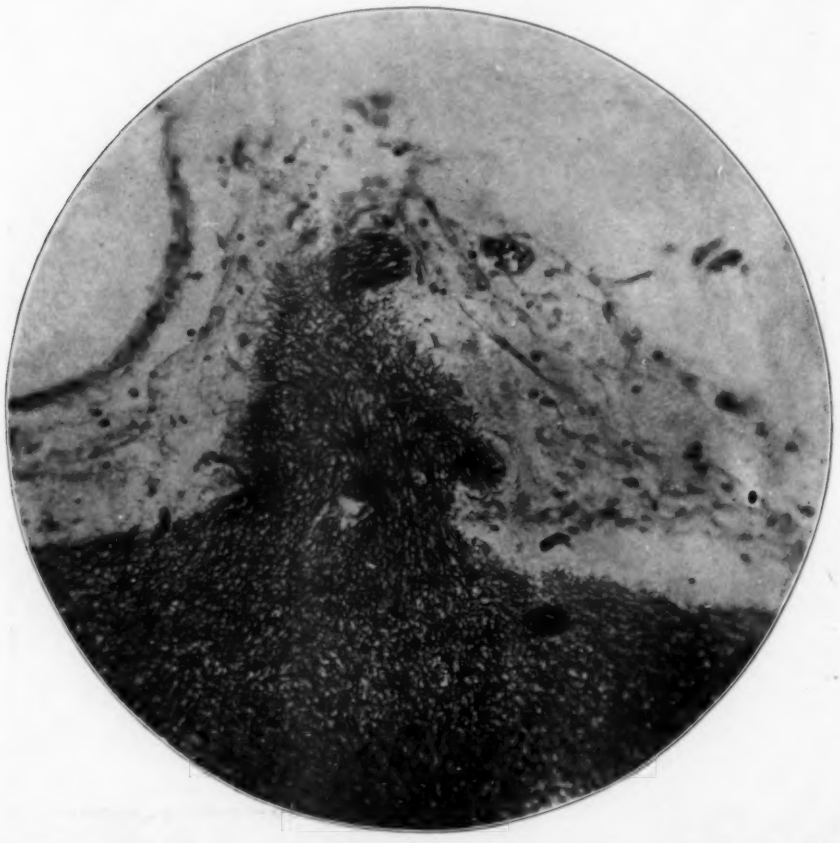

Fig. 8.—Section from the pons. Here a more massive migration of glial elements leads to protrusion of neuroglia into the pia-arachnoid. Photomicrograph; × 180.

shallow and the gyri extremely flat. The precentral gyrus was fairly well defined from the postcentral area by a deep central sulcus. The gyri in front of the precentral sulcus were indistinct because of meningeal thickening, adhesions and atrophy of the gyri. The postcentral gyrus was poorly defined and the parieto-occipital lobes were markedly atrophied. Here the normal markings of the cortex were entirely lost. Instead there was a stippled, corrugated, granulated hob-nail surface. The vessels were freely movable

over the surface (Fig. 1). On incision the cortex of the occipito-frontal lobe was found to be only about 2 to 3 mm. thick. At the occipital pole it was reduced to parchment-like thinness (Figs. 2 and 3).

Left Hemisphere: This was similar to the right. The sylvian fissure was well defined but shallow, exposing a poorly developed island of Reil. The middle cerebral artery was in the sylvian fissure and appeared to be patent throughout. There was no first or second frontal convolution but the third was present. The portion corresponding to the first two presented a granular, stippled surface

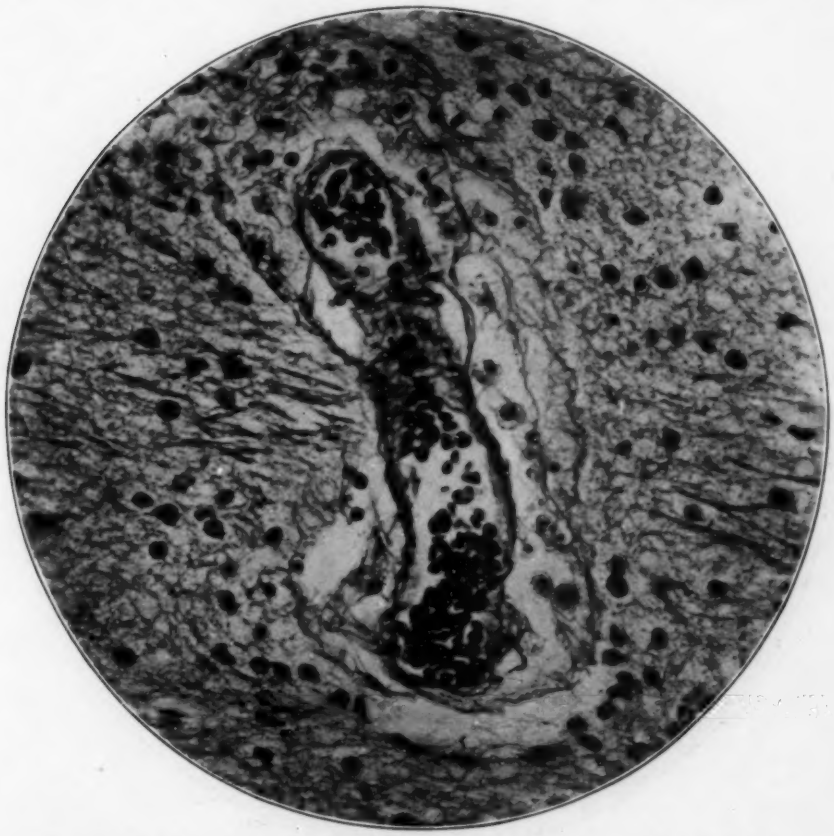

Fig. 9.—Section from the precentral convolution of frontal lobe showing marked distention of the adventitial space of the blood vessel. The granular (Gitter) cells found in the adventitial space point to a still active destructive process. Photomicrograph; × 400.

covered in places by thickened meninges. The first temporal gyrus was well developed; the second and third were absent, the latter being replaced by the granular cortical structure. The gyri on the base of the temporal lobe were absent, but the hippocampal gyrus was normal. The parieto-occipital lobes were similar to those on the opposite side. The orbital surface of the frontal lobe was normal.

The cranial nerves were normal.

The ventricles were markedly distended in all their divisions and with the exception of the frontal lobes left nothing but a shell of the cerebral hemisphere. The basal ganglions were well developed; the corpus callosum was present; the septum pellucidum was thin, stretched and fenestrated. The mesencephalon, pons, medulla and cerebellum appeared to be normal. The hypophysis was small, the sella shallow; the blood sinuses and middle ear were normal.

Microscopic Findings.—The thickening of membranes over the dorso-lateral surface varied. Thus, over the frontal lobes where the leptomeninges were

Fig. 10.—Vessel surrounded by a zone of dense gliosis. The adventitial wall of the vessel is infiltrated by small lymphocytes. Photomicrograph; × 200.

edematous, the subarachnoid space was distended and infiltrated with elements characteristic of a subacute inflammatory process. The changes in the pia-arachnoid, pointing to a more or less recent inflammatory process, were characterized by macrophages with or without enclosures and with or without vacuolated cytoplasm. Endothelial cells grouped in fairly large numbers about blood vessels constituted an important feature in the cellular infiltration. They were recognized by their large vesicular nuclei, poorly staining chromatin material and abundance of cytoplasm. Small lymphocytes were also seen

in fairly large numbers. Fibroblasts giving off delicate processes formed in places a delicate network. Plasma cells with eccentric nuclei, deep staining chromatin and faintly staining cytoplasm were rare. The blood vessels were much dilated and congested and showed thickening of the adventitia (Fig. 4).

A striking change in the leptomeninges was seen in the wide parieto-occipital area. The transition from the subacute inflammatory process in the frontal meninges to the chronic fibrous thickening over the parieto-occipital area was abrupt. About 5 mm. posterior to the central fissure the leptomeninges were

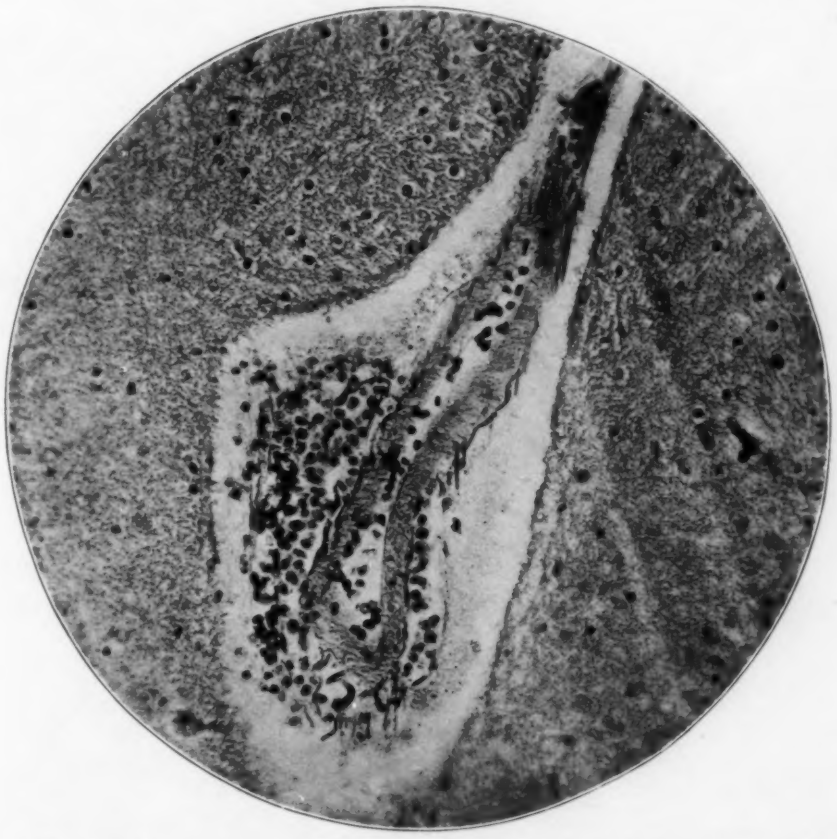

Fig. 11.—Section from the midbrain at the level of the inferior quadrigeminate bodies showing extensive adventitial infiltration of the vessels with mononuclear elements. Photomicrograph; × 240.

represented by a thick fibrous structure poor in cellular elements in which it was impossible to distinguish the pia from the arachnoid (Fig. 5). In some places extremely short pial processes could be traced into the cortex where blood vessels penetrated, but the majority of cortical vessels were apparently cut off by the meningeal cicatrization; others were strangulated by glial changes at a point just below their entrance into the cortex and in their deeper course were often obliterated and converted into a narrow band of connective tissue.

Strangulation and obliteration of blood vessels led to sclerotic changes in the cortical and subcortical substance. These zones of sclerosis corresponded to the areas supplied by obliterated vessels (Fig. 5).

There was a transition between these two types of meningeal alteration over the temporal lobe, particularly over the third temporal convolution. There was increase in fibrous elements with moderate mononuclear cell infiltration. Of interest was the fusion of the cortex with the pia-arachnoid alongside closed or partially closed vessels brought about by a most unusual migration of glial

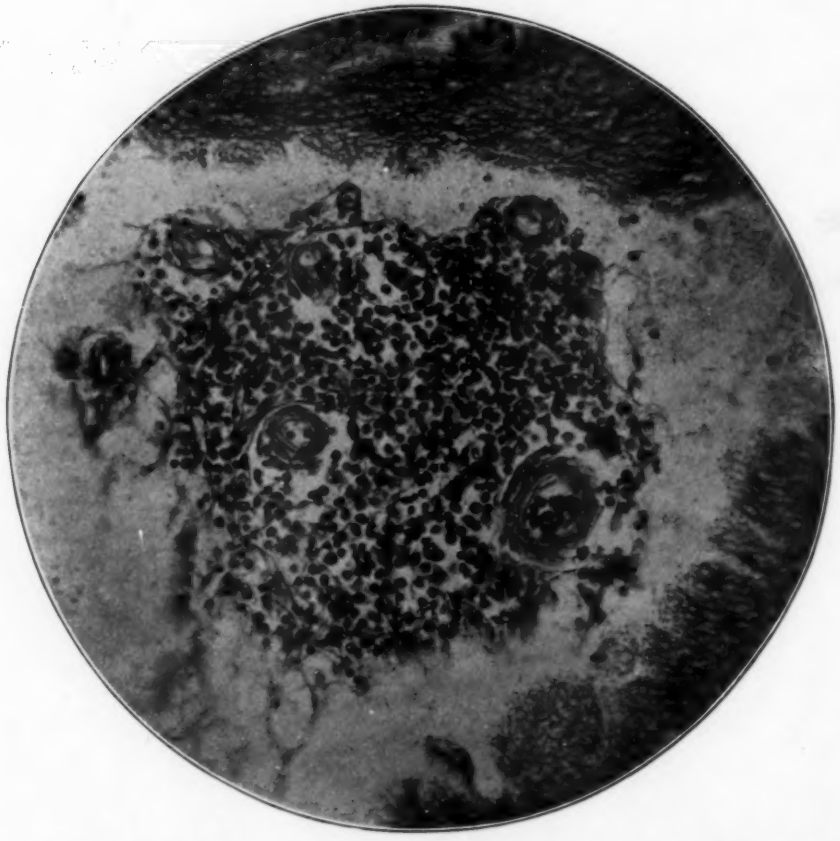

Fig. 12.—Section from the pons, showing a group of small vessels enveloped by a large mass of small lymphocytes. The adventitial walls of the vessels appear to be thickened. Photomicrograph; × 240.

elements into the pia-arachnoid. In this way the boundary between the mesodermal pia-arachnoid and ectodermal glia-supporting tissue was effaced. Thus individual glial cells could be seen migrating into the pia-arachnoid, their processes extending over the subarachnoid space while the cell bodies were still in the peripheral zone of the cortex. Also larger groups of glial cells were noted streaming into the pia-arachnoid membrane, alongside a partially obliterated blood cell (Figs. 6 and 7).

This reaction of glial cells to anemic conditions resulting from obliteration of blood vessels is thus clearly demonstrated and is in full accord with the frequently expressed opinion that glial elements usually react to circulatory disturbances in the central nervous system by proliferation.

This feature is most pronounced where the inflammatory process is most recent, as in the pons. There massive migration of glial elements gives rise to herniation of neuroglia into the pia-arachnoid (Fig. 8). Such a process was thought and is still considered by many writers to be impossible. But Saxer,[9]

Fig. 13.—Pons; vessels with distended perivascular space filled with serous exudate and a few lymphocytes. Photomicrograph; × 240.

in studying syringomyelia, called attention to glial infiltration of the pia. He even described the transformation of glial cells, on reaching the pia-arachnoid, into macrophages and other phagocytic cells. Also Strauss[10] has demonstrated involvement of the pia by an infiltrative subependymal glioma.

9. Saxer: Anatomische Beitrage zur Kentness der sogenannten Syringomyelie, Ziegler's Beitr. z. pathologischen Anatomie, **20**:332.

10. Strauss, I.: A Glioma Infiltrating the Pial Membrane, New York M. J. 1908, p. 1079.

Apparently the inflammatory process was primary where it now presents the more chronic features. Next, the inflammatory process involved the temporal lobe and finally the frontal lobes.

Cortex: The lesions of the cerebral cortex corresponded to the chronicity and character of the disease in the meninges. In the parieto-occipital lobes, where the meninges were fibrous, the cortex and subcortical substance were only a thin layer of scar tissue. Only here and there one could see a poorly preserved ganglion cell (Fig. 5).

The cortex of the frontal lobe presented an entirely different picture. Here many ganglionic elements were preserved, but the large motor cells were few in number and badly damaged. Important was the evidence of recent degenerative and inflammatory change. Large collections of fat globules crowding the adventitial space pointed to an active destructive process in the brain substance. Vessels with perivascular small round cell infiltration and with gitter cells crowding the adventitial space were further evidence of progressing inflammation (Fig. 9). Of particular interest were the vessels surrounded by zones of marked gliosis (Fig. 10) as well as wedge shaped zones of sclerosis resulting from the strangulation of the blood vessels by the cicatrizing pia-arachnoid.

In the mesencephalon at the level of the trochlear nerve was evidence of recent trouble—vessels with well defined adventitial infiltration. The cells found in the adventitial space were mostly small mononuclears, with a few plasma and endothelial cells (Fig. 11). In the pons was an area in which a group of small, probably newly formed blood vessels was enveloped by relatively large collections of mononuclear cells (Fig. 12); also vessels with serous exudate into the distended adventitial spaces (Fig. 13) and a limited number of perivascular hemorrhages were found.

Spinal Cord: Though it appeared that the ventral gray columns on one side were poorer in ganglion cells than on the other, it was difficult to establish a primary inflammatory process in this area.

COMMENT

The observations recorded in the foregoing present abundant evidence of an encephalitic process early in the life of the infant. The extensive destruction of the cerebral cortex and eleven months of the post-natal life not marred by any illness lead to the conclusion that the destructive process, resulting in the porencephalic defect, must have begun in the latter part of the intra-uterine life period and remained active and slowly progressive after birth.

That the physical development of the child was practically uninfluenced by the enormous loss of brain structure is most striking. This is best explained by the lack of early disease in the basal ganglions, midbrain, pons and medulla.

The opinion that the process was inflammatory and not traumatic or developmental is well borne out by the microscopic findings. Chronic and subacute lesions were found in the leptomeninges and underlying hemispheres, subacute and more recent lesions in the midbrain, pons

and medulla. Wherever the meningeal lesions were of the chronic fibrous type, complete strangulation of blood vessels resulted in diffuse and pronounced atrophy of the brain. The parenchyma was replaced by wide sclerotic zones, with fibrous bands.

In the temporal region, the pia-arachnoid was in process of fibrinous organization, shown by the presence of many fibroblasts and numerous mononuclear elements. Findings here indicate that wherever circulatory disturbances occur in the brain tissue, the destruction of brain parenchyma is accompanied by proliferation of glial elements. The thickening and cicatrization of the pia-arachnoid lead to strangulation of the blood vessels, producing ischemic zones of softening and sclerosis in which proliferation of glial elements and migration along closed blood vessels in the direction of the pia-arachnoid membrane are readily seen. This unusual process is of interest because it may help to a better understanding of changes that occur in multiple sclerosis, gliosis and scar formation in the central nervous system.

The histologic findings in the more recent lesions, especially in the region of the frontal lobe (mononuclear leukocytes, fibroblasts, plasma cells and macrophages) speak for a still active pathologic process. So probably the disease, started in the parieto-occipital region, spread to the temporal lobe, thence to the frontal region and finally to the mesencephalon, pons and medulla.

Repeated attempts to find the causative agent of the encephalitic process failed. Nevertheless the changes in the brain were clearly inflammatory, placing the case in the Strümpell group, so aptly named by Sachs—polio-encephalitis corticalis cerebri.

CONCLUSIONS

1. This case, although characterized by marked atrophy and diffuse sclerosis, can be considered one of porencephalus.

2. The atrophy and porencephalic defect were due to an inflammatory process, beginning as a meningo encephalitis in the parieto-occipital lobes.

3. The meningitic process led to strangulation of the blood vessels, producing anemia, degeneration of the parenchyma, proliferation of glial elements and finally to scar formation in the cortex.

4. Glia cells proliferate whenever blood vessels are occluded and migrate along the closed vessels toward the seat of active inflammatory process.

5. The migration of glial elements into the pia-arachnoid is well established by the findings recorded and supported by the observations of Saxer and Strauss.

6. The inflammatory process, primary in the meninges of the parieto-occipital lobes, slowly progressed to the temporal, then frontal lobes and finally to the midbrain and hind brain.

7. The strangulation of blood vessels causing sclerotic changes in this case is an important finding in relation to the pathology of disseminated sclerosis.

ARTIFICIAL PNEUMORACHIS IN THE TREATMENT OF ACUTE INFECTIONS OF THE MENINGES

A PRELIMINARY REPORT*

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BUFFALO, N. Y.

Injection of air or oxygen into the cerebral ventricles and subarachnoid space has become a recognized and valuable procedure in neurologic and surgical diagnosis.

As a therapeutic measure, injection of oxygen or air into the subarachnoid space by means of lumbar puncture, producing an artificial pneumorachis, has been found of considerable value in the treatment of some of the acute infections of the meninges.

During a small epidemic of meningococcus meningitis in 1916-1917 a few cases were found in which the purulent spinal fluid was obtained with difficulty owing to adhesions or blocking of the vertebral canal. Injections of half strength normal saline solution diluted the fluid and broke up adhesions, permitting a free flow of the purulent fluid. In some of these cases oxygen was injected with the result that an additional quantity of purulent fluid could be evacuated before injecting the antimeningococcus serum. In other cases the air or oxygen was used alone with the same favorable results in evacuating the fluid.

The technic employed is the ordinary procedure of lumbar puncture with the patient lying on the side. After removal of as much fluid as will flow through the needle, the oxygen is injected by means of a 20 c.c. Luer syringe. The syringe is filled by allowing the pressure from the oxygen tank to force back the plunger of the syringe. Sterile gauze is interposed between the gas cylinder and the syringe during the filling process. The oxygen is injected slowly, using from 5 to 10 c.c. as measured on the syringe. Removal of the syringe allows the gas and fluid to escape in a frothy mixture. Injection of the gas is repeated and the head and shoulders slightly elevated to allow the gas to reach the cerebral ventricles. This results in additional fluid escaping when the syringe is removed. In the meningococcus cases the serum is injected after this flushing-out process.

In these cases the oxygen probably acts only as a mechanical agent in displacing the fluid and opening up secluded pockets of adhesions.

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* Read at the Forty-Seventh Annual Meeting of the American Neurological Association, Atlantic City, N. J., June, 1921.

That the oxygen actually reaches the ventricles is shown by the increased amount of fluid forced out, the changed percussion note over the lateral ventricles, and the visible proof seen in the illustration in which the cerebral ventricular space is shown filled with oxygen which was injected by lumbar puncture.

The illustration shows that the gas has spread over the cerebral cortex in the subarachnoid space and fills the cisterna magna. Radio-

Ventriculogram showing lateral, third and fourth ventricles filled with gas. Oxygen was injected by lumbar puncture.

grams are thus of service in showing the patency of the foramina of communication between the ventricles and the subarachnoid space and in localizing pockets of purulent fluid walled off by adhesions.

THERAPEUTIC EFFECT OF OXYGEN AND OTHER GASES

The therapeutic effect of oxygen or other gases in contact with the meninges or ventricular ependyma is difficult to estimate. The fact

that different gases are absorbed at varying intervals of time suggests that some therapeutic value might be placed in the use of them. No harm has resulted in repeated injections of oxygen in the meningococcus cases. The absorption is fairly rapid as fresh amounts can be injected daily at the times of giving the serum. The patients are usually too ill to move to the roentgen-ray room for pictures at regular intervals.

In tuberculous meningitis repeated injections of oxygen has appeared to produce favorable results in a few cases. Whether it was the contact of the oxygen with the meninges or the repeated lumbar puncture with forced drainage that gave favorable results in three cases is impossible to determine. In three of twelve cases of tuberculous meningitis a complete recovery resulted, and the patients are well after an interval of a year or more. While the actual diagnosis of tuberculous meningitis may be questioned in these three cases, owing to absence of tubercle bacilli in the spinal fluid, the cases were in all other respects clinically tuberculous meningitis. The spinal fluid in each case showed a high cell count of lymphocytes, clear fluid and escaping with considerable pressure, a well-formed fibrin pellicle on standing; increase in globulin and albumin and a negative Wassermann reaction. One of the children had been under treatment for several years with Pott's disease; another had scrofulous glands in the neck which had been operated on; while in the third case the mother had a tuberculous history, although the child showed no other clinical signs of infection, aside from the meningitis.

None of the patients showing tubercle bacilli in the spinal fluid have recovered although in most of them the oxygen injections produced temporary improvement and even return of consciousness for a few hours.

In lethargic encephalitis less value should be placed on any form of intraspinal treatment owing to the deep-seated location of the lesions in most cases. One case, however, of the epileptic-maniacal type in which the lesion was probably a meningo-encephalitis responded to the lumbar puncture withdrawal of the spinal fluid and oxygen injection. In one other case of the lethargic type recovery from the deep lethargy followed promptly the injection of 15 c.c. of oxygen.

The average amount of oxygen injected has been from 10 to 15 c. c. The amount actually left in the spinal canal is difficult to estimate as a considerable portion escapes with the frothy fluid. In the case shown in the ventriculogram, the amount retained in the spinal canal and ventricles was over 20 c.c.

No ill effects or alarming symptoms have resulted from the oxygen injections. When the larger amounts of gas are retained in the spinal canal it is better to avoid too sudden elevation of the head and filling

the cerebral subarachnoid and ventricular cavities too suddenly. A slight dyspnea, due to bulbar irritation, has occurred when the filling has been too rapid, but the symptoms have not been alarming.

During the past five years a total of sixty-four cases have been treated by the intraspinal injection of air or oxygen, as shown in the table.

DISEASES IN WHICH INTRASPINAL INJECTION OF AIR OR OXYGEN HAS BEEN USED DURING PAST FIVE YEARS

Disease	Recovered	Died	Total
Meningococcus meningitis	23	7	30
Pneumococcus meningitis	0	5	5
Streptococcus meningitis	0	4	4
Typhoid meningitis	0	1	1
Tuberculous meningitis	3	9	12
Lethargic encephalitis	8	2	10
Poliomyelitis (acute)	2	0	2
Total	36	28	64

Of the total of sixty-four patients treated by artificial pneumorachis twenty-eight have died. At first glance this does not appear to differ materially from the usual statistics in such cases. In the meningococcus meningitis cases only the severe and apparently unfavorable cases were treated by this method. The mortality of 23 per cent. compares very favorably with the statistics of all cases of meningococcus infection in which the antimeningococcus serum is used alone.

The mechanical effect of the gas in opening secluded pockets of infection has undoubtedly prevented relapses as none of the patients who have recovered have had a recurrence such as occasionally occurs in other cases.

The experience in the few cases enumerated shows that oxygen may safely be injected in the spinal canal and that it acts as a mechanical and possibly as a therapeutic agent in the acute meningitides.

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DISCUSSION

DR. M. ALLEN STARR of New York asked whether in the literature or in Dr. Sharp's experience there were any fatal cases and what the proportion of deaths was.

DR. ERNEST SACHS of St. Louis asked Dr. Sharp whether in these cases of epidemic meningitis the patient had first had the Flexner serum and whether at the time when the oxygen was injected the organisms had disappeared from the fluid, because sometimes even when it is difficult to get some of the fluid out, the organisms have disappeared, and it raised the question as to whether the oxygen itself offered any therapeutic help. It seemed to him that the type of case in which oxygen might help would be with an anaerobic organism, as Dr. Ayer had said; using oxygen with an aerobic organism, which the

meningococcus is, would hardly seem to inhibit its growth and act therapeutically; it may act mechanically.

Dr. Colin K. Russel of Montreal asked whether the two patients with poliomyelitis quoted by Dr. Sharp died or recovered with the usual paralysis.

Dr. James B. Ayer of Boston thought the subject of pneumorachicentesis was of much interest. The procedure had of course been carried out by Dandy, and in Boston Dr. Cruthers had safely used it at the Children's Hospital for diagnosis. Concerning its use as a therapeutic procedure, he did not feel that it had been demonstrated that air or oxygen is of specific benefit in meningitis, and it should be shown that it is the air, and not simply irrigation, that is of value. If the three cases of meningitis mentioned were actually tuberculous, which unfortunately was not proved, then the success of this method must be admitted, in that such types almost always are fatal. It appeared that in the hands of Dr. Sharp the method had proved safe, and should therefore be further employed, although with great care and with due attention to pressure changes during the procedure.

Dr. Edward A. Sharp of Buffalo, in closing, stated that in some of the cases pneumoracentesis had been performed merely as a last resort. In 1916, two patients died, but they were practically moribund at the time. The injection of oxygen had no effect in hastening the end; in fact, in none of the cases had the oxygen injections been responsible for a fatal outcome. The value of the oxygen is probably mechanical in its effect of removing more fluid.

Since hearing Dr. Ayer's paper last year the idea of using the cisterna magna puncture to inject the oxygen in those cases in which adhesions prevent the fluid reaching the lower spinal dural sac seemed practicable.

Dr. Sharp said that oxygen probably had very little, if any, effect in destroying the germs, the effect being largely mechanical. The contact of the oxygen with the inflammatory area might have a formidable therapeutic effect by increasing the vascular supply.

Opening the abdomen in cases of tuberculous peritonitis had occasionally been followed by long remissions in the symptoms, and the favorable results had been attributed to the contact of the peritoneum with the air or oxygen. It was possible that a similar theory might be applied to the meninges.

Replying to Dr. Sachs' question, Dr. Sharp said the Flexner serum was used in all the meningococcus cases. In the severe cases the irrigation was employed at the first injection. When the fluid was thick the irrigation was done with saline solution and then followed by the oxygen. At the hospital patients had done better when the subarachnoid space had been fairly free from pus before the injection of Flexner's serum. The saline irrigation and the oxygen had apparently had no effect in lessening the activity of the serum. In all cases the serum was used early. As stated by Dr. Sachs, when the fluid became thick and difficult to get out the organisms were not found.

Previous to using the oxygen method at the Ernest Wende Hospital, a number of patients had relapses after apparently clearing up, due to pockets of pus which were not reached by the serum. With the oxygen method, breaking up adhesions, the relapses have not occurred.

Dr. Sharp's experience in the poliomyelitis cases had been the same as that mentioned by Dr. Russel. The two patients mentioned were in the hospital while the work was in the experimental stage, and the injections were performed as a routine procedure. The effect of removing the larger amounts of fluid by displacement with oxygen was apparently favorable.

A SYNDROME OF THE VISUOPSYCHIC CORTICAL AREA—BASED ON STABILE HALLUCINATIONS AND DEFECTIVE VISUAL ASSOCIATION IN A SANE PERSON *

H. H. HOPPE, A.M., M.D.
CINCINNATI

The discussion of this subject presupposes that in normal persons the stimuli perceived by the visuosensory center are transmitted to the visuopsychic areas, are there compared with previous visual pictures and are recognized as similar or dissimilar. There they are stored, but the manner in which they are stored is unknown.

Memory pictures stored in the visuopsychic center can be recalled to the field of consciousness either by the will or by stimuli coming from other cortical areas through the association tracts. Stimuli coming from without through the visual organs can also recall these memory pictures, and the flow of these stimuli is always from the visuosensory to the visuopsychic area. Never under normal conditions can the process be reversed. Stimuli from the visuopsychic or other cortical areas cannot stimulate the visuosensory areas. If this were not true, we could never be certain of the objective reality of our visual impressions.

Under abnormal conditions this flow of stimuli is reversed—memory pictures stored in the visuopsychic areas, stimulate the visuosensory area and are then projected into space. They are then known as hallucinations.

In considering syndromes of the visual area of the brain, we must distinguish between lesions of the visuosensory area and lesions of the remainder of the occipital cortex, namely, the visuopsychic area. The classical sign of the former is simple hemianopsia unassociated with other signs and of the latter, mind-blindness.

We must also distinguish between lesions which are cortical and those which are subcortical, between irritative lesions and destructive. Lesions, however, which we are called on to localize are not always strictly localized to either the sensory or the psychic areas, and the same lesion may be irritative first and destructive later on. Thus, irritative lesions of the calcerine area may cause simple or elementary hallucinations, such as flashes of light or color, which precede the development of the hemianopsia. If the lesion is more widespread we may have more complex hallucinations projected into the failing field

* Read at the Forty-Seventh Annual Meeting of the American Neurological Association, Atlantic City, N. J., June, 1921.

of vision, but with cortical destruction of the visuosensory area, the only sign is blindness. The projection of visual hallucinations presupposes an anatomically normal visuosensory area.

Hemianopsia, alexia and agraphia are not, strictly speaking, an occipital lobe syndrome, but are caused by a subcortical destruction of the association tracts beneath the gyrus angularis.

An irritative lesion of the visuopsychic area may be one-sided and may show itself in the appositive visual field as one-sided hallucinations. This may occur occasionally with intact vision, as in Peterson's well-known case. Cases of hallucination in hemianopsia, in which the visions are projected into the blind field, are proof of the foregoing assertion.

Mind-blindness, which is the typical sign of a destructive lesion, requires a destruction of both visuopsychic areas or their association tracts from the visuosensory area. I wish to call your attention now to another syndrome of the visuopsychic area, which is probably caused by an irritative lesion. This syndrome presents these three factors: (1) persistent stabile visual hallucinations with intellect, speech and general memory intact, (2) normal visual perception, and (3) disturbed visual association. The occurrence of stabile visual hallucinations in persons who are mentally normal is not frequent. Instances which come under our observation are considered sufficiently rare to excite comment, and we are apt to remember them as cases of unusual interest. In these persons single objects are usually seen and then only on a single occasion. I do not refer now to epileptics with visual aura or to cases of hemicrania.

A woman physician, a patient of mine, with a tendency to hysterics would have attacks of great fear produced by the vision of a white cat walking across her consultation room. Nearly everyone knows of some person or has heard of some one who has had a vision.

Guerney [1] asked *27,737* persons whether they had ever had an hallucination and obtained affirmative answers from 12 per cent. of those questioned. The impression, however, is made that these persons had experienced the illusion or hallucination only once.

Stearns,[1] in his study of 500 cases of mental disease, observed in the Boston Psychopathic Hospital, says that hallucinations are rarely observed in sane persons even though they are psychopaths. In his group of 500 cases, twenty-four were demented and had arteriosclerosis; only three had hallucinations, and two of these were insane.

Hallucinations occurred least frequently in the group of those not insane, as only 6 per cent., four out of sixty-five, were affected. Two were hysteric patients who had had one hallucination each, and the other two were children with pavor nocturnus.

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1. Stearns: J. Nerv. & Ment. Dis., 1915.

I do not hesitate to place a rather unusual clinical case on record because: 1. A person may have visual hallucinations of distinct groups in the form of definite pictures extending over a long period of time and still be of normal mind. 2. The case offers a definite visual memory field syndrome which may have a definite localizing value.

There are comparatively few observations on record of persistent visual hallucinations in persons not insane. Most of these occur in cases of hemianopsia with hallucinations in the blind field, or in cases in which fleeting hallucinations preceded the occurrence of blindness in destructive lesions.

AUTHOR'S CASE

History.—E. H. F., a widower, aged 91, at the age of 61 had a right-sided hemiplegia with aphasia. He recovered completely in three months. About one year ago he had a stone removed from the bladder by the suprapubic route. He lost his fortune at the age of 75, but made another. Within the last six years the patient has put into operation a new large enterprise, which is successful. He shows good business judgment and is daily engaged in directing large enterprises.

Present Illness.—About four weeks ago the patient began to have "visions." These occurred when he was wide awake, either when sitting in his library or when resting in his bedroom. These "visions" occurred daily and annoyed the patient. They produced no emotional reaction, and they were recognized by the patient as not being real.

These visual hallucinations can be divided into four groups:

1. The patient on looking out of his window at a neighboring lawn always sees a large red brick house. As soon as he fixes his attention on the house, it begins to move toward him, comes up to the window and then disappears.

2. On looking out of the window the patient sees a lake with ships on it. One of these ships begins to sail toward the window and when it comes abreast of the window it disappears. Both of the hallucinations can be shut out by closing the blinds.

3. Groups of men, white and black, swarm into the library from the hall door—at times the figures are whole, at others, there are only heads. Again the heads and faces appear distorted—the noses are at times missing, heads seem to be moving about, severed from their trunks. These figures are always in motion.

4. Well dressed women walk into his bedroom. They pay no attention to him, walk past the foot of his bed to a cheval mirror, arrange hat and veil, adjust their clothing and then walk out of the room. The apparitions are always silent—they never speak and never pay any attention to the patient.

These visual hallucinations never vary from these groups. They are not present when the patient is motoring or when he goes about his warehouses, offices or stock farm. They never produce any emotional reaction except annoyance and the fear that he might be considered insane. He knows that they are not real. They occur only when he is wide awake—never in the half conscious state preceding or following sleep. The patient does not dream.

Physical Examination.—The patient was physically well. The arteries at the wrist were palpable, but soft. The blood pressure was: diastolic 95, systolic 140. The aortic valve sound was only slightly accentuated. Pupillary reaction

was normal. Ophthalmoscopic examination showed that the disk, retina and vision were normal. There was no defect in the function of any of the cranial nerves. The upper extremities were normal for sensation, muscular power and reflexes. The lower extremities were normal, there was no change in the reflexes, muscular power was normal. There was no trace of the hemiplegic attack of thirty years ago. Urinalysis was negative. His intellect was normal. He was a keen business man and personally attended to large affairs. He showed a normal interest in local, national and international affairs. He was oriented for time and place, was very cleanly in his person and careful of his dress and appearance. His memory for events both recent and past was good. There was no speech defect. The patient could read and understand what he read. There was no amnesic or optic aphasia.

Visual Tests.—There was no loss of vision, the patient could see and recognize any object which was shown him; knew the use of it, and could describe it. If shown pictures of single objects he recognized them and named the colors correctly. He was shown pictures, for instance, of men and fish and made no mistake as to the nature and color of the object.

Association Visual Memory Test.—The patient was shown many pictures and was not able to interpret the meaning of them. When asked to describe the picture in words he would read the advertisement or explanation printed below, he could not grasp the story told by the picture. In one picture, which was an advertisement of the Cragmoor Sanitarium, he pointed out the buildings but could not recognize the mountain range, finally saying that he saw another building where the mountain range was portrayed. When shown another picture which was used as an advertisement for a breakfast food, he could not interpret the story conveyed by the picture. When shown groups of men in other pictures he was not able to tell what scene the picture was intended to portray. Men, fish or other printed objects, if single, were recognized. In other words, visual memory association was lost. Analytic and synthetic comprehension of pictures was impossible. Stereoscopic vision was normal. The patient was also oriented as to space, recognized his surroundings, was able to go about the house, go out to and enter his automobile, and walk about his extensive farm without any difficulty.

He had no difficulty in placing or recognizing the proper position of objects in space. Spatial orientation of an object seen showed that there was no faulty projection. He was accurate in judging distances of objects in space. Perception of movement and light perception were normal. He was not color blind. Psychic blindness in the ordinary sense was not present, for the patient both named and recognized the nature and use of objects.

CAUSES OF HALLUCINATIONS

This case is reported in order to discuss whether we may not have here a new syndrome of an irritative lesion of the visuopsychic area, composed of: (1) persistent stabile visual hallucinations; (2) normal visual perception for simple objects; (3) disturbed visual association, and (4) normal mentality.

This patient had no lesion of the visuosensory area, nor were the association tract or tracts between the visuosensory area and the visuopsychic area broken down, for there was no mind-blindness, no loss

of orientation in space and he had no trouble in judging the distance of objects from the body. His color perception and color sense were good.

There was no optic aphasia, no alexia and no hemianopsia which would indicate that the subcortical tracts coming to the occipital lobe from the region of the gyrus angularis were intact.

The patient did not recognize his hallucinations as real, they did not affect his judgment and emotions, which would indicate that his remaining cortex and their association tracts were intact and normal in function.

His inability to explain the meaning of pictures showed a lack of normal visual memory for the purpose of association of previous memory pictures. Visual memory, synthetic and analytic, was defective. This is a function of the visuopsychic area.

Hence, reasoning by exclusion, the clinical picture is produced by an irritative and perhaps partly destructive lesion of the visuopsychic area caused by senile arteriosclerotic changes. I believe, therefore, that this case proves that we can have a visual syndrome characterized by persistent stabile hallucinations, with normal vision perception, without mind-blindness but with defective visual analysis and association, in a person who is not insane.

Another interesting feature which this clinical case brings out clearly is that a person may have persistent stabile visual hallucinations which may persist over a period of months and not be insane. I believe that the proof of this assertion is made clear by a careful reading of the case.

Most of our knowledge of the genesis of visual hallucinations in sane persons has been obtained from the study of hallucinations in hemianopsia. It is a prerequisite for the occurrence of hallucinations in cases of hemianopsia that the visuosensory area should be normal. The conduction apparatus for vision and the subcortical tracts need not be normal, for visual hallucinations may occur in the blind, where blindness is due to diseases of the conduction tracts. A lesion, therefore, which destroys the conduction tract on one side and at the same time exerts an irritative influence on the visuopsychic area of the occipital cortex can prodce visual hallucinations in the blind field. As there are not many organic lesions which can simultaneously destroy and irritate, cases of this kind are relatively infrequent. Henschen was the first to hold that these cases of hemianoptic hallucinations were not due to lesions of the visuosensory area but to irritative lesions of the visuopsychic area and that they, therefore, have a certain localizing value.

It has long been known that visual hallucination can be caused by destructive lesions of the occipital lobe, but in these cases the visual hallucinations always precede the occurence of half-sided blindness.

Thus Climenko [2] reports a tumor of the occipital lobe with progressive hemianopsia. When the headache was very severe the patient saw many colored animals and objects pass before his eyes. "He had hallucinations of myriads of rapidly moving objects and various colors."

Hallucinations occurring in the hemianoptic field either preceding or following the loss of vision prove that hallucinations can arise from mechanical pressure or from irritative lesions of the visuopsychic area.

Hallucinations always point to an abnormal condition of the central nervous system. This, however, need not necessarily mean a destructive lesion; and while hallucinations usually occur in the insane, their presence is not pathognomonic of insanity even though they persist for weeks and months. The crucial point of difference is what becomes of the hallucinations—Does the consciousness of the individual recognize the hallucination as such or does it accept it as real?

The result of the hallucination depends on the sum total of consciousness and the integrity of the other sensori-psychic areas. If the latter are normal, the visual error or hallucination is corrected. Consciousness, attention and correct functioning of the other senses enable people to bring proof that the hallucination is not an objective sensory impression.

Fleschig [3] explains this by saying that if only one sensoripsychic center is involved the individual remains sane, but if various sensory centers together with the posterior association areas are involved the patient becomes confused and is the victim of an hallucinatory psychosis.

DISCUSSION

Dr. Hugh T. Patrick of Chicago asked Dr. Hoppe whether the visions always approached from the same side. He also asked whether the patient was right-handed and in which field the visions occurred.

Dr. M. Allen Starr of New York thought that this was a very interesting case on account of its duration.

He called attention to the fact that hallucinations of shorter duration might occur under the influence of poisons. He had a patient brought to him, a very intelligent Protestant woman, who had awakened one evening with very bright hallucinations of the Virgin Mary with the infant in her arms, appearing at the foot of her bed. She was extremely alarmed and was very much perturbed at this appearance which persisted constantly for four days, giving rise to the suspicion or fear on the part of the family that she was going crazy.

Dr. Starr could, however, find no symptom of mental aberration, no loss of memory, no delusional state, merely the persistence of this marked hallucination of vision. It was not attended by hallucinations of hearing and was not accompanied by any other symptoms, with the exception of a great sense of fear and alarm. He noticed that there was a great dilatation of the pupils,

2. Climenko: J. Nerv. & Ment. Dis., 1915, p. 828.
3. Fleschig: Gehirn und Leele.

dryness of the throat and of the skin, and on inquiry found that her family physician had prescribed for her an ordinary purgative or laxative pill. He had written the prescription, which was shown to him en masse, putting in 3 grains of belladonna, to be divided into twelve pills, and the probability was that the druggist in putting it up, managed to get all 3 grains of belladonna into one pill, and that the hallucination was due to poisoning by belladonna. The event proved to be so, because as the days went on, the dilatation of the pupils subsided, the dryness of the mouth and skin subsided, and she gradually lost this particular hallucination and was well. She has had no recurrence for six or seven years. Therefore, a hallucination may last a considerable time—in this particular case four days continuously—simply as the result of accidental poisoning.

Dr. H. H. Hoppe of Cincinnati, in closing said that he had found, in looking through the literature on the subject, that none of the patients was subjected to visual association tests; mention was made that the individuals had visual hallucinations only.

Answering Dr. Patrick, he said it was not a case of hemianopic hallucination, as the man's vision was normal. The patient was right-handed.

News and Comment

SECOND ANNUAL MEETING OF THE ASSOCIATION FOR RESEARCH IN NERVOUS AND MENTAL DISEASES

The second annual meeting of the Association for Research in Nervous and Mental Diseases will be held at Hotel Commodore, New York City, Dec. 28 and 29, 1921.

PROGRAM

MULTIPLE SCLEROSIS

Presidential Address.....Dr. Walter Timme

SYMPTOMATOLOGY

General Symptomatology.....Dr. Bernard Sachs
Special Symptomatology Study.....
.....Drs. J. W. McConnell, Williams B. Cadwalader, George Wilson
Hyperkinetic Motor Phenomena.....Dr. Frederick Tilney
The Location of Lesions with Respect to Symptoms.....Dr. E. W. Taylor
The Spinal Fluid in Multiple Sclerosis.....Dr. James B. Ayer
Aural and Vestibular Manifestations.....Dr. Isidore Friesner
Ocular Manifestations.....Dr. Ward A. Holden
Psychotic Symptoms.....Dr. Sanger Brown, 2d, Dr. T. K. Davis
Psychologic Factors.....Dr. Smith Ely Jelliffe
Clinical Endocrine Studies.....Dr. Walter M. Kraus, Dr. Irving H. Pardee

STUDIES IN ETIOLOGY

General Etiology.....Dr. Lewellys F. Barker
Bacteriologic Studies in the Pathogenesis of Multiple Sclerosis.....
.....Dr. Oscar Teague
Pathologic Studies in the Pathogenesis of Multiple Sclerosis.....
.....Dr. George B. Hassin
Endocrine Studies, Macroscopic and Microscopic.....
.....Dr. Hubert S. Howe, Dr. Myrtelle M. Canavan

STUDIES IN PATHOLOGY

Pathologic Considerations in Multiple Sclerosis.....Dr. William G. Spiller
Axis Cylinder Changes in Multiple Sclerosis.....Dr. J. H. Leiner

INCIDENCE AND STATISTICS

Geologic and Ethnologic Features..Dr. C. B. Davenport, Dr. C. B. Esterbrook
Multiple Sclerosis as Revealed by the Draft.....Dr. Pearce Bailey
Statistical Data.....Dr. I. S. Wechsler
Bibliography.....Dr. Frederick Tilney

Abstracts from Current Literature

ACUTE CIRCUMSCRIBED EDEMA AND RELATED CONDITIONS.

H. QUINCKE, Med. Klin. **17**:675 (June 5) 1921.

In this rather extensive article the author reviews the subject of angioneurotic edema and allied conditions. It is his belief that the disease is not as uncommon as usually supposed, owing to the fact that the usual case is transient, not serious, and rarely requires the services of a physician.

Quincke has observed from sixty to seventy cases. In the last thirty-six cases 50 per cent. occurred in males and 50 per cent. in females. The more complicated and atypical cases appeared in the females. The youngest patient was 13, the oldest 68. The majority of patients had their first attack when between 21 and 30 years of age. The average duration of the disease was five years, the minimum a single attack, the maximum thirty years. The length of the single attack varied from a few hours to six days, but usually lasted a day and a half. The eruption may be so fleeting as to go unobserved.

The length of intervals between attacks varies greatly—from twenty-four hours to years. At times the attacks occur at irregular intervals. They may occur in series with a long period of remission. Two such cases are briefly reported, the first patient having had two attacks in twenty-one years, the second patient two attacks in four years, the last attack consising of a series occurring daily, usually most severe between the hours of 7 and 11 p. m. In another case the interval was twenty-six and eighteen years, respectively.

Usually with increasing age the attacks of edema become lighter and more infrequent, or may stop entirely. Periodicity is common; the attacks occur not uncommonly at certain times of the month, on certain days, on rising in the morning, after the noon-day meal, etc. A close relation to migraine, epilepsy, hydrops genu intermittens, certain colics, types of erythema and neuralgias is noted in the paroxysmal type of the attacks.

The etiology of the disturbance is not yet clear. Menstruation, psychic excitement, fatigue, loss of blood, heavy meat consumption, constipation, articles of diet, such as strawberries, chemicals, such as caffeine, aspirin, etc., are mentioned. One patient gave the cause as sea-bathing, two as warm baths, three as pressure or other mechanical measures; excessive sunshine is also mentioned. Climatic conditions are also referred to, but usually the associated change in the diet plays a more important part. In one of the cases reviewed by the author the cause of the edema was thought to be due to the absorption of toxins as the result of a cholecystitis, the edema disappearing on the removal of the gallbladder.

As a rule, patients having an angioneurotic edema appear healthy. During an attack, except for the local discomfort, little disturbance is noted. In only three cases were any febrile signs noted.

In 39 per cent. of the cases general nervousness was present; 13 per cent. showed a familial tendency to urticaria or acute edema. The majority of patients were anemic and sedentary and reacted well to change in mode of living and exercise. Robust out-of-door persons were, however, also affected.

In Quincke's edema, the swellings vary from 2 to 10 cm. in diameter. The margin is not sharply defined, but blends into the surrounding skin. The

color may be unchanged or slightly red, there being no increase in local heat. The edema is firm and does not pit; the patient usually complains of a feeling of fulness, occasionally burning, rarely itching. Besides the skin, the subcutaneous tissue may be involved.

Any grade of hyperemia may be present up to a distinct wheel formation with temperature, itching and pain, but no proportional constitutional symptoms. At times there may be local tenderness before the appearance of the edema. Quincke's edema and urticaria, while usually very distinct conditions, may show any possible admixture. In four cases, urticaria had existed for years prior to the appearance of the circumscribed edema. Urticaria is frequently seen as an associated condition in the familial type of edema. The edema may also be associated with erythema multiforme, erythromelalgia, and Raynaud's disease.

Frequency of involvement of various parts is given: face, 80 per cent.; eyes, 64 per cent.; lips, 53 per cent.; legs, 44 per cent.; hands, 41 per cent.; arms, 25 per cent.; tongue, 22 per cent.; prepuce and scrotum, 20 per cent.; thighs, 19 per cent.; pharynx, 17 per cent.; mouth, 8 per cent.; gastro-intestinal organs, 5 per cent.; esophagus, 3 per cent., and periosteum, 5 per cent.

Occasionally symmetrically situated parts are affected one after the other. There is no rule, however, as any or all of the areas mentioned may be affected in one attack or in succeeding attacks. In no case in this series was the larynx involved; rarely the mouth, pharynx, stomach or intestines are involved, and, if so, only in association with some other region.

Ten cases are cited by the author to illustrate the unusual distribution which the edema may take. Several atypical cases are also reviewed. In one interesting case in this series, the patient, a female, had for months a swelling of the eyelids, first one, then the other, usually occurring in the morning. Finally, under calcium lactate treatment, the condition cleared away. Another case was that of a patient who had swelling of the face for eight months, first diagnosed as erysipelas.

Migraine was found to be frequently associated with Quincke's edema (seven cases). In some cases the headache precipitated the attack, in others no apparent relation was present. In one case of severe migraine which had existed from the age of 10 to 50, there developed at the latter age, with practically every headache, a swelling of the face and lips, or an edema occurred without headache.

Quincke is of the opinion, as is also Bürgi, that migraine is a manifestation of meningeal edema, exactly as angioneurotic edema is a swelling of the skin or mucous membranes. On this basis Quincke believes that one is able to explain the great variety of cases of migraine, and also the numerous cerebral focal signs, as edema of the meninges may occur anywhere as it may on the skin.

In a case of left-sided headache noted by the author, in which the headache lasted for three days, there was an associated twitching of the right side of the face and right arm with weakness, lasting several days, but always passing away. Cases of hemiplegia with migraine have been observed, and Quincke has seen two patients with monoplegias. These cases he believes are due to edema of the pia with pressure or possible associated cortical involvement.

Hydrops genu intermittens, edema of the bursae and tendon sheaths are also discussed by the author. Cerebrospinal fluid exudates are also noted, and the author reports three cases in which, associated with the menstrual period, there occurred a serous meningitis.

Recurrent neuralgias are mentioned as a possible result of edema. One case of recurring sciatica, according to Quincke, was due to localized edema of the lumbar plexus. Recurring myalgias are also due to this cause. In some of the studied cases colloid changes in the muscle were thought to be the cause, but Quincke personally believes that the condition is due to an edema of the intermuscular connective tissue fibers.

Various other types of edema are mentioned by the author under the heading of circumscribed edema. In one case reported by Neuda and Gagstatter, edema of the mucous membrane of the bladder was noted.

Edema due to other causes, such as nephritis, cardiovascular conditions, mechanical reasons, etc., is reviewed by the author. In discussing the physiology of this condition the author reviews several theories based on ion concentration.

The transitory character, the periodicity and the location point to a nervous origin, as do also the usual history of nervousness, familial tendency, psychic emotions and the known similarity to migraine and epilepsy.

Under the heading of treatment the author has little new to offer, diet, hygiene, etc., playing the usual important rôle. Other therapeutic measures are mentioned but none that has been used with uniform success.

MOERSCH, Rochester, Minn.

THE ANATOMICAL AND CLINICAL SYNDROMES OF THE CORPUS STRIATUM. J. LHERMITTE, Neurol. Bull. 3:163 (May) 1921.

The author presents a comprehensive study of this important structure. He insists clearly and logically that there is no evidence to justify the assumption that the corpus striatum is in any sense regressive in structure or function. We have come to our present knowledge of this subject by the anatomicoclinical route; physiologic investigation has been misleading and practically barren of constructive result.

The anatomoclinical syndromes are grouped under three headings: those in which the lesion involves the corpus striatum in its entirety; those which affect the striatum (putamen and caudate nucleus); and those which implicate the pallidum. The pallidum includes not only the two internal segments of the lenticular nucleus, but also, according to Ramsey Hunt, the large-celled neurons scattered over the putamen and caudate nucleus. The pallidal syndrome would appear to be determined not only by destructive or degenerative alterations affecting the globus pallidus, but also by those that reach the pallidal element carried to the interior of the striatum along with the purely striate neurons (striopallidal system). It will be seen that Lhermitte's studies are governed chiefly by histologic considerations.

Under syndromes of the entire corpus striatum the author considers: first, the progressive lenticular degeneration of Kinnier Wilson; second, pseudosclerosis; third, the senile and presenile pseudobulbar syndrome of striate origin which differs from Wilson's disease in the type of destructive process. In the former there is gross vascular alteration; in the latter "even though the loss of nerve and neuroglial elements have a paravascular origin, the lumen of the capillaries, veins and arteries remain quite normal"; fourth, striate syndromes caused by presenile lesions of the striatum and pallidum (C. and O. Vogt); the senile parkinsonian syndrome.

Syndromes of the striatum include: first, the syndrome of Cecile Vogt, with marbled appearance of the striatum; Little's disease, with congenital and

regressive spasticity alone; second, dysmyelinization (C. and O. Vogt); general rigidity with progressive terminal athetosis; third, Huntington's chronic progressive chorea, the result of "atrophic cortico-striate degeneration." In this type the author feels that true adiadokokinesis is exhibited, and therefore that this symptom is probably not the exclusive property of cerebellar lesions, but that it should be accorded a place among the striate signs; fourth, the author includes acute lesions of the striatum—Sydenham's chorea. In this and in Group 3 we find diminished muscular tone and the hypertonicity of other striate syndromes is replaced by hypotonicity. Thus a coordinator rôle is assigned to the striate body; fifth, lesions (hemorrhages or softening) in the region of the striatum.

Syndromes of the pallidum bring into the discussion the much mooted question of Parkinson's disease, particularly paralysis agitans according to the views of Ramsey Hunt.

All the diseases of the striate body have certain symptomatic elements in common. They are exclusively motor; there is universal muscular rigidity, clearly distinct from pyramidal spasticity; there is marked diminution of spontaneous voluntary movements. To this are related the disturbances of the normal facial expression and gesturing that accompany language and thought.

The author's conclusions are embodied in his closing sentences. "What appears most clearly when we leave anatomico-clinical syndromes and attempt to picture the functions of the corpus striatum as a whole is that the influence of the purely motor functions of this structure are very different from that of the cortico-bulbo-spinal system (pyramidal system). The latter has to do with the most highly differentiated and delicate movements. They may be considered the most intellectual and consequently the most human. All the more rudimentary functions are carried out by the striate system. They are the older movements of the vertebrate series, and, continuing the metaphor, more animal in character.

"From the physiopathologic point of view, those conditions which bring about more or less complete destruction of the corpus striatum affect functions which are essentially automatic and which are only slightly influenced by the cortex. Walking, the synkinetic movements of defense and of spatial orientation, the mimicry of gesture and facial expression, swallowing and phonation are all functions of an automatic character. It is these automatic functions, long gone from the plane of consciousness, which, as the Vogts pointed out, are affected by disease of the corpus striatum.

"Everything leads us to the general conception that the motor corpus striatum and the sensory optic thalamus correspond to the precentral motor gyrus and the postcentral sensory gyrus which receives conscious sensations. And, further, the corpus striatum and the optic thalamus control the superior automatic motor system, while the cortical centers dominate and control the voluntary motor system."

STRECKER, Philadelphia.

THE POST-TRAUMATIC HEADACHE SYNDROME. R. BENON and B. KERBRAT, Schweizer Arch. f. Neurol. u. Psychiat. **8**:184, 1921.

Some time previously Benon called attention to a post-traumatic nervous condition, which he designated by the name traumatic headache or the syndrome of post-traumatic headache. He states that the fundamental and initial symptom of this syndrome is headache, and that the various nervous and psychic manifestations which accompany it are secondary and determined by it. The

syndrome may for this reason be separated from post-traumatic neurasthenias and the less severe forms of traumatic dementias. Three case histories are reported:

The first patient was a soldier, 21 years of age, who, in 1914, suffered a cerebral commotion as the result of an explosion of a projectile. He was unconscious for a period of from twenty-four to forty-eight hours. Prolonged treatment at various stations was undertaken with indifferent results. In November, 1916, he was seen by the writers. At this time the fundamental symptom was headache. This was present continuously, but fluctuated in intensity. Violent paroxysms of pain, particularly in the occipital and sincipital regions with radiation toward the temples, occurred. The pain was described by the patient as being like a heavy weight on the head; when the pain became intense it semed as though terrific blows were being struck. During exacerbations, there was vertigo and a profound alteration in character; the patient assumed imperious attitudes, came and went, gesticulated, and was very much agitated, struck at doors, and jostled his comrades. He became fatigued easily, and there was some confusion of ideas and difficulty in remembering. The family history was essentially negative except that his mother was of a nervous temperament. Previously the patient had been rather timid, but alert.

The second patient was a male servant, 16 years of age, who, in 1912, had been kicked in the left frontal region of the head by a horse. At that time a diagnosis of cerebral commotion and fracture of the frontal bone was made. After five days he was able to be up and about. He was examined from time to time until October, 1917. The dominant symptom was headache, which was continuous but fluctuated in intensity. He was exceedingly irritable, struck at objects, spoke in a loud voice, and was rather menacing toward those about him. This agitation would last several hours, at which time he would feel that he would never again be able to live like his comrades. The more he became demoralized, the more violent he would be. With a lessening in intensity of the headache, there was a return of optimism. At times he suffered from nightmares. Although this headache had continued for a period of five years, it had not changed in intensity. The family history was negative except that a sister committed suicide.

The third patient was an infantryman, 31 years of age, who, in September, 1916, suffered a cerebral commotion with multiple contusions from the explosion of a projectile and burial under a mass of débris. Consciousness was lost at once, but returned after an hour. There was extreme ringing in the ears, and great pain in the head and in the chest. His treatment at a number of different hospitals was unsuccessful. The patient was seen by the writers in September, 1917, when he was found suffering from a continuous headache present on awakening and persisting until night. It did not hinder sleep, however. The pain was always great but was subject to violent and intolerable paroxysms. Its location was in the neck, with radiation to the temples. The patient described the sensation as though a machine were in continuous movement in the head. During the paroxysms he was more irritable and at the least contradiction felt pain in the chest and experienced a sense of choking. The family history in this case was negative, except for nervous crises occurring in the mother.

These three observations are typical examples of the post-traumatic headache syndrome. The asthenic manifestations, alterations in character, hypo-

chondriac and melancholic tendencies are all in direct relation to the intensity and violence of the headache. The headache usually has its seat in the occiput and is extremely severe.

WOLTMAN, Rochester, Minn.

SOME PROBLEMS CONFRONTING THE ASSOCIATION. OWEN COPP, Am. J. Psychiat. **1**:13 (July) 1921.

This indictment of the present status of mental medicine combined with definite working plans for a bright future is too valuable not to be read by every neurologist and psychiatrist.

The counts in Dr. Copp's indictment are the indifference of the public and physicians to giving mental patients an even chance with sufferers from other forms of illness, the lack of adequate psychiatric instruction in medical schools and the appallingly inadequate facilities for research in mental diseases.

Plans for remedying present conditions are given in considerable detail. They involve prevention through school inspection and the teaching of special groups of children; through the increase in instruction to student physicians. An available agency of prevention as well as treatment is the bringing of patients with mental cases to general hospitals where in the psychopathic wards they can receive emergency care and short and intensive treatment for "mental states not recognized as such by patient or his friends and for other conditions which are curable and not disturbing to the general hospital regime." This is essential in order to get good advice for the mental patient from specialists in all departments of medicine; and it is by the treatment of these specialists that many physical causes can be removed.

There follows an unusual summary of the possibilities of treatment in hospitals. In the first place, there is treatment in the university clinic (capacity 100) or in the endowed private hospital (capacity 100 to 300) of special cases for special purposes and combined with intensive research. In the second place, or as a substitute, classification can be obtained in metropolitan (psychopathic) hospitals (capacity 100 to 200) with rapid turnover of patients and close affiliation with medical schools.

In the third place, there is treatment in the district (state) hospital with a maximum of 2,000 patients, the most important single agency in the treatment of mental disease and so the place at which the "maintenance of the highest standards is imperative." This hospital must as a practical matter not only receive patients directly, study them as do the clinics, but also be equipped to hold patients for indefinite periods and to provide a farm colony. This hospital "will minister to more than 90 per cent. of mental patients throughout the longest illness; will have exclusive opportunity in most cases, so that any deficiency means to the patient proportionate deprivation of the best hope of restoration to health or alleviation of disability. It must strive to meet every requirement of every type of mental hospital because the necessity of every kind of service is sure to arise without possibility of other aid in many cases." This district psychopathic hospital both from the humane and the scientific standpoint must be protected from politics. Its surest safeguard is decentralization—the supervisory rather than the control method; the supervisory method being investigation, comparison, conference and wise publicity. No new building for mental patients should be built without first having its trustees and superintendent appointed in order to plan for its economical medical administration.

A discussion of the internal affairs of such a district (state) hospital is of great interest and brings new ideas. The problem of the dual control from the business and medical sides is attacked by a division of the hospital's functions into four spheres—clinical, scientific, communal and administrative, the latter comprising the nonmedical administration relating to business affairs separate from the care of patients. Each of the spheres so marked must be regarded as coordinate with the others; each should be headed by a director experienced in its special functions, the director of the administrative having technical education and full opportunity for development in a satisfying life career. The field of inter-relationships between all four departments must be left to a medical superintendent, who must act as harmonizer, have final decision subject to appeal to the governing board, but be big enough to give each director full scope in his own department.

The last point in logical order made by Dr. Copp is rehabilitation with a field inside the hospital consisting of systematic training and habit formation and including the occupational homestead and a field outside comprising community psychiatric service.

To one interested in legislation which concerns mental diseases, to any one planning a new hospital, this paper will offer help in big and in little things. It is a sad thing to see money, desperately needed by afflicted human beings, wasted because of ill considered plans.

BOND, Philadelphia.

OCULAR DISTURBANCE IN ENCEPHALITIS LETHARGICA. P. P. WAAIDENBURG, Am. J. Ophth. **4-8**:580 (Aug.) 1921.

The diagnosis of epidemic encephalitis in many cases cannot be made from the presence of any constant syndrome of clinical symptoms, but the ocular symptoms have proved helpful in fixing the diagnosis in many instances, especially because the disease is characterized not only by the frequency of the well-known isolated or combined nervous paralysis which we meet in other infectious and nervous diseases, but because supranuclear disturbances of the fibers or centers of associated movements often precede the other symptoms.

Three other diseases give differential diagnostic difficulties: (a) nona in which bulbar symptoms are absent, (b) premature paralysis agitans which proceeds more slowly and without inclination to sleep, and (c) bulbar paralysis in which changes of the ocular muscles and pupillary changes are absent. The author elaborates on the cases described by Nonne and Economo and then presents three cases. The ocular symptoms only will be discussed. Ptosis is the most frequent and is more or less fugacious and is usually combined with diplopia; the degree is varied from a paresis to a paralysis. Visual disturbances are due to a retrobulbar neuritis and is manifested by a ring scotoma which is not always isolated but often connected with a central scotoma or enlargement of the blind spot, and it often has inconstant and capricious shapes. Ring scotomas have also been described in multiple sclerosis, hereditary neuritis, syphilis, retrobulbar neuritis, affections of the optic nerve arising from clinical causes, and glaucoma. Paresis of accommodation is also a frequent symptom, but it is also found in syphilis, botulism and diphtheria.

Paresis of convergence is less frequently found. Anisocoria is a cardinal symptom and present in about 20 per cent. of cases. The degree of dilatation is inconstant; the reaction is present but slow, the amplitude slight. The author explains this phenomenon in the following way: "Because the nuclei

of the sphincter must be small, the hypothesis is not hazardous that all supranuclear reflex paths which attain this nuclear from about the same direction must lie very close together in some place. If we accept a partial or total destruction of these paths, whether or not accompanied by a slight irritation or paresis of the sphincter nucleus, many facts can be explained in a plausible way; that is, the long duration of the condition, restoration in a different sequence, the different combinations of symptoms, etc. Thus a normal reaction of convergence can be combined with an isolated paralysis of accommodation, and the other combination mentioned."

Paresis of the associated movements, too, are often disturbed and are often combined with total or partial contraction of the homonymous parts of fields of fixation; vertical paralysis predominates over horizontal.

Nystagmus is also a frequent symptom and is due to a disturbance of the vestibular nuclei or the connection fibers between them and the nuclei of the oculomotor or the tonic centers of the conjugated movements.

Concerning localization of nuclei and fibers—we have not advanced much further than the hypothesis: the paralysis of convergence speaks for the existence of a center of convergence placed in the median nucleus of the oculomotors. Besides the oculomotor phase, the facial, which lies at a greater distance, is often attacked, so that we are forced to accept the theory that focii lie more dispersed rather than that they are in continuous connection. We are, therefore, forced to accept an eclectic action for the verus for definite cerebral parts.

DeLong, Philadelphia.

THE CONNECTIONS OF THE VOMERONASAL NERVE, ACCESSORY OLFACTORY BULB AND AMYGDALA IN AMPHIBIA. C. Judson Herrick, J. Comp. Neurol. **33**:213 (Aug. 15) 1921.

The vomeronasal organ, or organ of Jacobson, has long been a puzzle to comparative anatomists and physiologists. Generally present in air-breathing animals, this portion of the nasal organ may contain a considerable part of the specific olfactory membrane, or in microsmatic forms like man it may be vestigeal. When well developed, this portion of the olfactory sensory epithelium has its own nerve, the vomeronasal nerve, which terminates centrally in a special part of the olfactory bulb, the accessory bulb. These structures are absent in fishes and appear in definitive form in the frog and in most reptiles and mammals. The first appearance of this apparatus was apparently correlated with the opening of the posterior nasal aperture, and recent observations of Broman suggest that it is primarily a mouth-smelling organ, that is, a special portion of the nose adapted for smelling odorous mediums within the mouth.

In the frog a special olfactory tract leaves the accessory olfactory bulb to terminate in a differentiated nucleus which is regarded as the precursor of the mammalian nucleus amygdalae. The other connections of the anuran amygdala are: with the opposite amygdala through the anterior commissure, with the adjacent pyriform lobe, with the medial olfactory areas, with the habenula, and with the hypothalamus, the latter by descending and ascending fibers of the olfactory projection tract of Cajal. Through these connections the amygdala of the frog serves as a center for the correlation of odorous excitations derived from the mouth cavity with those derived from the air outside and of both of these with taste and other visceral functions of the hypothalamus.

This appears to be the inception of the amygdaloid complex, which primitively in amphibia was differentiated under the influence of specific sensory excitations coming from the vomeronasal organ. So far as the preexisting nervous pathways connected with the ventrolateral area of the cerebral hemisphere conducted excitations physiologically congruous with those from the vomeronasal organ, these were integrated into a morphologic unity as we see it in the amygdala of the frog. The amygdaloid complex, having been thus integrated under the dynamic influence of vomeronasal excitations, further complication of the system progressed in higher vertebrates in various directions by the incorporation of other related sensory systems, with perhaps profound modification of the functional aspect of the complex as a whole. So great may be this deviation from the primitive physiologic pattern in some cases that the suppression of the vomeronasal organ, as in man, or even of the entire olfactory system, as in the dolphin, does not destroy the integrity of the surviving components of the amygdaloid complex, which retains its individuality in modified form.

Other parts of this paper include a description of the corresponding parts of the brain in urodele amphibians, in which the first stages of this differentiation are recognizable, but without coming to consummation in the complete elaboration of either vomeronasal organ or amygdala. There is also a discussion of the morphogenesis of the strio-amygdaloid complex and a brief review of the comparative anatomy of this apparatus.

AUTHOR'S ABSTRACT.

TECHNIC OF NERVE SURGERY. K. WINFIELD NEY, Ann. Surg. **73**:37, (July) 1921.

The author, having had, within the short span of fourteen months, more than 1,500 peripheral nerve cases under his immediate care, has presented on the basis of this experience a system of procedure and technic in the operative treatment of peripheral nerves, based on physiologic indications. In the suture of mixed nerves, torsion of the nerve trunk results in the physiologic misdirection of nerve fibers, in which motor fibers are directed down sensory channels and sensory fibers down motor channels, resulting in a physiologic loss of function. The author suggests several methods for the prevention of torsion. Nerve defects are considered, and their method of correction is taken up in detail. Improvements in technic have almost entirely eliminated the necessity of grafts for filling defects. Most of the grafts used in his clinic were eventually removed, because of nonregeneration, and it was found possible to overcome most of these defects by the methods described and to accomplish end-to-end suture.

The technic of suture in complete and partial divisions is presented with many illustrations to clarify the text. Physiologic interruptions are classified according to the factors responsible for their production: (1) external compressions due to scar tissue, bone callous, or any form of pressure; (2) thickening and infiltration of the nerve sheath; and (3) interstitial fibrosis (perifunicular).

A definite surgical technic is shown for the correction of each condition. One case of ischemic paralysis of the leg which had existed eighteen months without improvement is reported: blood vessels were exposed and found to be practically normal; the sciatic nerve was opened revealing extensive peri-

funicular adhesions; the individual bundles were then separated and the sheath left open. There was a return of function in practically all the muscles within three months.

The author lays special stress on the influence of technic on final functional results, in which the amount of scar tissue is diminished by the avoidance of tissue trauma, infection and by careful hemostasis.

In conclusion, he states: "The methods suggested in this paper for the prevention of torsion have given fairly satisfactory results, but now and again we find cases in which in spite of these precautions taken to prevent torsion it has evidently occurred, and there has been a diminished functional return in certain muscles. In some cases a return of function has been so perfect in most of the muscles that it requires a careful examination of individual muscle function to determine motor imperfections, and no doubt much of the resulting disability will be expressed only in a diminished disability to perform certain skilled movements, and there is a possibility that even these minor motor defects may in time be corrected in many individuals by reeducation and substitution movements."

AUTHOR'S ABSTRACT.

PILOUS VIRILISM AND ITS ASSOCIATION WITH GLYCOLYTIC INSUFFICIENCY. C. ACHARD and M. J. THIERS, Bull. de l'Acad. de Méd. **85**:51 (July 19) 1921.

The first portion of this paper is both historical and descriptive of hirsutism as it sometimes appears in women. According to Apert, who is quoted, there are three fundamental characters which it possesses, namely, pilosity of a special topography, adiposity, and disorders of the genital functions. One can observe diverse anomalies of the sexual characters, notably mammillary atrophy, hypertrophy of the clitoris or a masculine voice. But all these associated phenomena are inconstant.

We are badly enough informed regarding the mechanism which, in the physiologic state, determines the growth of hair in certain regions in the case of the male adolescent. Its pathologic development in the infant of either sex or in the adult female is even more obscure.

Besides the frequent anomalies in the genital sphere, uterine malformations—such as bicornous, unicornous or double uterus, ovarian cysts, etc.—suprarenal lesions are frequent. It is difficult to define the nature of the genital disturbance which directs this hirsutism. It may be due to an overactivity of the suprarenal cortex. But every suprarenal lesion does not have this result, and one can find among infants suprarenal tumors without coincident premature development.

The writer then gives in detail the history and necropsy findings in a case of hirsutism associated with a glycosuria which was not permanent. The hypertrichosis began at 9 or 10 years of age. The patient had had regular menstruation which lasted to the unusual age of 60, and she had had one pregnancy. At the age of 69, following a severe trauma, she showed a glycosuria which disappeared under mild regulations of diet. He contrasts this case of hirsutism associated with glycosuria with others of recent reporting (Emile-Weil and Plichet; Tuffier and Guemes, Laignel-Lavastine and Boulet). Regarding their menstrual or reproductive functioning, there is no uniformity in the series whatsoever. Also the anatomic variations in the genital sphere are as variable as in so-called simple hirsutism. Again, the suprarenal capsules in some, but not in all, instances have presented lesions.

He calls attention to an increase in the cholesterin content of the blood in these cases. Finally, the hypertrichosis made its appearance at very variable dates—in the case reported before the onset of menstruation, at the menstrual epoch in another, between two pregnancies in another and at the menopause in one case.

The degree or severity of the "diabetes" associated with hirsutism is also extremely varied, as the author shows by citing figures from the assembled cases, and he considers that glycolytic insufficiency is only an inconstant and variable element in feminine hirsutism of the virile type. He takes its occasional appearance as proof of the participation of several organs in a pluriglandular syndrome.

DAVIS, New York.

NEUROTROPIC FILTRABLE VIRUS. PAUL HARVIER, Paris méd. **2**:485, 1921.

This interesting review is summed up as follows:

"Epidemic hydrophobia, poliomyelitis and encephalitis belong to the same group of infectious diseases, brought on by filterable virus having an elective affinity for the nervous system."

There are analogies and differences between these three diseases which are summed up thus:

These viruses exist in the general nervous system in a pure state. These diseases can be experimentally reproduced and transmitted from animal to animal by inoculation of an emulsion of the central nervous system or by the filtrate of that emulsion. The inoculation gives positive results, not only in the intracerebral path, but also in the ocular path, the peripheral nerve path and nasal path (after scarification or traumatism of the membrane).

The virus of encephalitis, alone, produces a specific keratitis. The virus ascends the nerves to reach the cerebrospinal axis where it grows during the period of incubation of the disease.

These different viruses act in a fashion analogous to physical and chemical agents. They resist light and desiccation; they are killed by heat of 55 degrees and by a certain number of antiseptics; they conserve their virulence in glycerin. They are destroyed by bile.

According to American bacteriologists, their culture in Noguchi mediums shows bacterial forms very closely related.

The inoculation of these cultures produces the experimental disease. The parasite may be determined in the central nervous system of those animals which succumb to the disease.

The relations of epidemic poliomyelitis and encephalitis are more intimate than those between these two and hydrophobia. The first two diseases are transmitted by the same pathway—the nasopharynx. The presence of their virus was shown in the nasal membrane and the nasopharyngeal secretions. The propagation of these diseases seems to be accomplished by the sick, the convalescing, perhaps the healthy persons living with or near these sick people and by "virus carriers."

However, besides the analogies, fundamental differences exist. These consist, above all, in the unequal receptivity of the various species of animals with regard to each of these viruses, in the contents of the serum of vaccinated animals and of cured cases, and in the particularities of immunity.

The serum of animals vaccinated against hydrophobia neutralizes in vitro and in vivo the virus of hydrophobia. Similarly, the serum of animals cured

of an attack of poliomyelitis neutralizes the virus of that disease. Epidemic encephalitis acts differently. The attempts at vaccination, of neutralization of the virus by serums, has given, up to the present time, only practically negative results.

A cross immunity between hydrophobia, encephalitis and poliomyelitis does not exist. Animals which have natural immunity to the virus of poliomyelitis are susceptible to hydrophobia.

The virus of encephalitis does not vaccinate the monkey against poliomyelitis. In a like manner, the virus of poliomyelitis does not vaccinate the rabbit against encephalitis.

(This article was extracted by P. H. from "Infections à Virus Filtrants" from "Nouveau Traité de Medecine at the Thérapeutique" published under the supervision of M. Gilbert and Carnot, vol. 10 [in preparation].)

KRAUS, New York.

ON THE BREAKING OF FAMILY—AND OTHER—TIES. KRAEPELIN, Ztschr. f. d. ges. Neurol. u. Psychiat., O. **63**:1-8, 1921.

Americans will find a contribution by Professor Kraepelin interesting, even if it seems rather remote in its way of dealing with its subject. This subject, the effects on a man of cutting him off from the different circles to which he was born, seems too big to be handled when it involves human relationships from family to nation.

Man as a social being is discussed. The author considers that, certain essentials being fixed by heredity, the development and direction of a person is largely controlled by those personal contacts which are most powerful in youth. Given some weakness of character to start with, the child and youth turns to the family for protection and help in trouble, and he turns with a sense of "belonging." It is the family that has the best chance to smooth his rough places, laugh him out of his conceits, and teach him the beneficial results of easy communication with his fellows.

Ordinarily when the child turning for help finds step-parents, he suffers. Something is gone for which the kindest treatment is not a substitute. He is one step removed from "his own people."

The effects on the personality will evidently vary according to the causes of the separation from the family or nation. On one side great external accidents such as death may do the disrupting; on the other side the personal peculiarities of a member of the family or oneself. There follows a rather formal account of the possible variations of separation in degree and in kind. It may be complete or incomplete. The illegitimate are affected in one way and the orphans in another; the bearer of a great name and a person marked by the commonplace and by poverty; the city merchant and the peasant. A father drunk may produce the effect of a father dead. There is a different problem with the only child and with the child surrounded by brothers and sisters. And the effect must be graded according to age; it is doubtful whether it is stronger in childhood or in puberty; at a certain age it is normal.

The evil tendency consequent on breaking natural ties at the wrong time is seclusiveness and self-investigation. The means with which to fight it is an expansion of social psychiatry. Schools must make a scientific study of the problem. States must study colonization. More attention must be paid in general to the effects on character of leaving, or being left by, one's own family, neighbors or nation.

BOND, Philadelphia.

PARKINSONIAN SYNDROMES. FERNAND LEVY, Gaz. d. Hôp. **94**:757 (June) 1921.

This is a review of the paper by Souques and the discussion, presented at the Paris Neurological Society in June, 1921. The meeting was devoted to the parkinsonian syndrome.

Introduction.—This syndrome is defined as follows: "Paralysis agitans is not a morbid entity, but a syndrome resultant on various causes acting on the same part of the brain."

Pathologic Anatomy.—The following theories of the location of the lesions were given:

(a) Ramsay Hunt: A lesion of the globus pallidus of the striate body. This lesion affects the pallidal system.

(b) Tretiakoff: This author believes the lesion is due to atrophy and degeneration of the cells of the substantia nigra.

(c) S. A. K. Wilson emphasizes lesions of the red nucleus.

(d) These theories are not at variance fundamentally according to Mirto, who regards the substantia nigra as a part of the globus pallidus, separated from it in the course of phylogenetic development.

(e) Foix believes that the lesion is a diffuse one of the striate body not limited to the globus pallidus.

The types of lesions include tumors, cerebral hemorrhage, cerebral softening, infections, lesions, toxic processes, etc. These lesions must, in order to produce the syndrome, affect either the striate body or the substantia nigra —which of these must be decided in the future. A more or less extensive involvement of the centers of tonus results (Souques).

Etiology.—(a) The syndrome cannot longer be considered a neurosis. Fear may play a part in precipitating the disease. It does not cause it but may, acting on suitable soil, bring it to light.

(b) Trauma is to be considered as a precipitating factor in a few cases. Souques, who saw a great many war cases, both of emotional and concussional shock, never saw paralysis agitans as a sequel.

(c) Infections are now the best established causes of the syndrome. Before the epidemic of encephalitis this was not generally recognized, though cases following scarlet fever, typhoid, measles, articular rheumatism, malaria and syphilis were reported. Epidemic encephalitis is the most frequent cause.

The syndrome comes on, as a rule, between the ages of 40 and 60, though it may occur in youth and childhood (3 years).

Symptoms.—The main symptoms are rigidity, tremor and loss of associated movements.

These symptoms are described in too much detail for review. Pain is briefly considered. It is present in about one sixth of the cases. Vasomotor disorders are not infrequent. The rest of the review is devoted to a section on clinical forms, diagnosis and treatment.

The clinical forms described are the classical type, a forme fruste with rigidity and no tremor and a forme fruste with tremor and no rigidity.

No essential difference exists, according to Souques, between the juvenile and presenile forms. Neither the section on diagnosis nor that on treatment contains information that is not familiar.

KRAUS, New York.

THE COURSE OF PARESIS. Friedrich Meggendorfer, Ztschr. f. d. ges. Neurol. u. Psychiat. **63**:9, 1921.

The author reports on the findings in over 1,200 cases of general paralysis of the insane. Concerning the incubation period, the author shows graphically the latent period between the syphilitic infection and the onset of the paralytic symptoms. With an infection in youth the latent period is long, paresis rarely setting in before the thirty-fifth year, twenty years after the initial lesion. With increasing age at the time of infection the incubation period of paresis grows shorter, until with an infection coming first at 50, a latent period of barely five years may be expected before the onset of the paralysis. Moreover, with a late onset the course is apt to be shorter and more stormy and lead to an earlier death. Cases of juvenile paresis due to congenital syphilis are, of course, excluded.

According to the author, and as is frequenty observed, paresis develops in cases of syphilis with the milder secondary manifestations; moreover, the treatment of syphilis by mercury and iodids seems to have a shortening rather than a lengthening effect on the latent period before the onset of paresis.

The infrequency of paresis in prostitutes is accounted for by the early age at which most of them are infected with syphilis, the prolonged latent period permitting many other opportunities for degeneration and death before the paresis has become established.

The tendency of paresis to occur during the fifth decade of life is strongly emphasized by the author, and in explaining this occurrence it is noted that this is the decade of involution, when the endocrine tissues are undergoing degenerative processes, the internal chemical processes are less vigorous, and the forces of resistance of the body are being broken down.

Finally, the much vexed question of the elective action of certain "strains" of the spirochete for the nervous system is taken up. Meggendorfer theorizes thus: A person, owing to disease or injury of the nervous system, becomes more susceptible to injury at this point. He becomes infected with syphilis, and the spirochete attacks the weakest tissue. He already has the "disposition to metalues." He develops paresis, and infects another with syphilis. The spirochete, causing a mild general infection, will localize in the central nervous system and produce paresis again. Thus the strain will be determined. In support of this theory he brings in the observation of Pilcz, that the Hungarians are prone to develop paresis, but that the person of foreign race, contracting syphilis on Hungarian soil, will be far more apt to develop paresis after infection with the spirochete, than he will be if infected in any other part of the world.

Freeman, Philadelphia.

CONVULSIONS IN EARLY INFANCY. A.-B. Marfan, Presse méd. **29**:633 (Aug. 10) 1921.

Convulsions of all types in this age-period are almost always accompanied by loss of consciousness. They tend to be repeated. The age itself is the greatest predisposing factor; most infantile convulsions occur in the first two years of life, particularly from the third to the sixth month.

The "complete, general, and regular" convulsion resembles that of ordinary epilepsy in its successive stages. The onset is oftenest instantaneous, the duration generally very short (a few seconds to a minute or two). Status is

not rare. Atypical tonic-clonic convulsions are more common than the typical in this age-period. They may be incomplete (tonic spasms without clonic, or clonic without apparent tonic), partial (usually affecting some portion of the face) or irregular (tonic and clonic spasms alternating variably).

Precipitating causes of infantile convulsions may be: (1) organic, almost any acute or chronic central nervous lesion; (2) symptomatic, an affection of another system, as in infections, intoxications, and sudden pyrexias from any cause; (3) a spasmophilic diathesis, with tetany (Marfan disputes Escherich's assertion that all cases with early infantile convulsions show signs of latent tetany between attacks); or (4) idiopathic.

Group 4 is losing in favor of Group 1 through more intensive and successful searching for signs of heredosyphilis. In the few residual cases that must still be called idiopathic, epilepsy is not necessarily to be expected as a sequel.

The question whether a convulsion is taking place sometimes arises with laryngospasm (a localization of tetany), which may or may not precipitate a true convulsion. Spasmus nutans may also be confusing.

In any type of infantile convulsion, the immediate and remote prognoses are both more grave if the tonic spasm is unusually marked or prolonged.

When phenylethylmalonylurea is used, Marfan recommends a large dose of ⅓ to ½ grain daily for infants under 6 months. He also states that boric potassium tartrate is badly tolerated.

HUDDLESON, New York.

ACUTE INFLAMMATORY TABETIC ARTHROPATHIES. A. LEMIERRE, M. LÉON-KINDBERG and P. N. DESCHAMPS, Gaz. d. hôp. **94**:901 (July) 1921.

These authors describe a case of acute inflammation of the left knee which proved to be of tabetic origin. After the patient had remained in the hospital a week a backward luxation of the tibia occurred. The knee and Achilles reflexes were absent on the unaffected side. The spinal fluid showed lymphocytosis.

The arthropathy was characterized by severe pain, frank local signs of inflammation and a general febrile reaction.

The authors comment on the relative rarity of the condition, the severity of the pain, the diffuseness of the swelling (from the ankle upward in their case), the suggestion of erysipelas in some cases, the transitory general febrile reaction and the bad prognosis. The prognosis is bad not only as to the local condition, but often also as to the general condition of the patient. After the febrile attack, the disease progresses rapidly and often ends in death. In some cases, however, this does not occur and the patient, after recovering from the acute attack, presents chronically, the classical picture of a tabetic arthropathy. Secondary purulent infection may occur.

Treatment by antisyphilitic drugs is not effective, since, as these authors point out, the damage has already been done. They suggest surgical interference in some cases, opening of the joint, and curetting of the necrosed tissues for the purpose of eliminating a dangerous focus of infection.

An interesting account of the pathology of these acute tabetic arthopathies is given. This emphasizes the syphilitic etiology and the difference of the findings from those of coccal forms of arthritis. The paper is interesting and thorough.

KRAUS, New York.

THE VASCULARITY OF THE CEREBRAL CORTEX OF THE ALBINO RAT. Edward Horne Craigie, J. Comp. Neurol. **33**:193, (Aug. 15) 1921.

In a previous paper the author described a standardized method for determining the relative vascularity of parts of the central nervous system and presented quantitative data on specific regions of the spinal cord, brain stem and cerebellum of the albino rat.

The vascularity of the cerebral cortex is of about the same order of magnitude as that of the cerebellar cortex. The vascularity of each of the five recognizable layers was measured in five different areas of the cerebral cortex. In all areas the lamina granularis interna (IV of Brodmann) is more richly vascularized than are the other layers, and the supragranular layers tend to be richer than the infragranular layers, a result of considerable interest in connection with current beliefs that the supragranular layers serve higher cortical functions than the infragranular layers. The author believes that the granular and supragranular layers are receptive and associative in function, while the infragranular layers give rise to corticofugal fibers. This suggests a comparison with the lower centers, where the sensory and correlation nuclei were found to be more richly vascular than the motor nuclei. The average vascularity of the five layers is the same in the occipital and temporal areas, and is only slightly less in the precentral region. The parietal area is distinctly richer than the others, while the insular cortex is much the poorest.

C. J. Herrick, Chicago.

THE PSYCHOPATHIC PRODROMAL SYMPTOMS OF EPIDEMIC ENCEPHALITIS. P. Chavigny and E. Gelma, Bull. de l'Acad. de méd. **85**:113 (July 26) 1921.

Since the work of Netter, Achard, Legre and Bremer, the frequency and variety of psychic symptoms in the prodromal period of epidemic encephalitis have been recognized. These symptoms are characterized by mental confusion, and behavior on an impulsive basis sometimes transitory and followed by post-confusional lacunar amnesia, and they appear before the onset of the commonly recognized usual symptoms of the disease. If a patient in the prodromal period, commits an unlawful act leading to arrest, and if a short time after his arrest, the confusion has disappeared, one can easily fail to understand the morbid nature of the felonious action and may believe that the subject is simulating an amnesia.

A young peasant became ill with fever, photophobia, frontal headache, stiffness of the neck, diplopia and spinal fluid findings in accord with the diagnosis of encephalitis, on the day following his arrest for the theft of a bicycle. He was in a confused state, was imperfectly conscious of his situation and was unable to recall the reason for his incarceration. The writers present the amnesia encountered in the case as legitimate and relate that he was declared irresponsible by reason of his state of consciousness at the time of committing the act. Psychiatrically there was not alone a post-confusional amnesia but confabulation with a mingling of true and false memories, of post-delirious ideas and fantastic interpretations. He remained unable to relate himself to the reality of the past in spite of reiterated explanations made before him.

A possible hypothesis of simulation may not be sustained for several reasons —the certitude of the clinical encephalitis which arose only a day following

the theft, the knowledge that disordered and extravagant acts are sometimes features of its prodromal psychic symptomatology, and the conviction that the correctness of the clinical psychiatry pictured, confusion, amnesia and confabulation was beyond the simulating powers of a peasant.

DAVIS, New York.

INTERNAL SECRETIONS AND THE NERVOUS SYSTEM. W. LÖFFLER, Schweizer Arch. f. Neurol. u. Psych. **8**:163-183, 1921.

It is extremely difficult to formulate any theory explaining the physiologic and pathologic processes going on in the organism that does not include the conception of internal secretions. The doctrine of internal secretions, however, has developed into a form of mythology in which the various hormones are enthroned and deposed, very much like the gods and demigods of the ancients, as the necessity of the writer demands.

The correlation taking place between various organs in the body may proceed according to nervous, chemical and neurochemical means. These distinctions, however, cannot always be clearly drawn. For example, as a purely chemical correlation may be mentioned the production of urea in the liver. An example of a neurochemical process would be the classical experiment of puncture diabetes.

The writer reviews the theories relating to various organs of internal secretion, such as the hypophysis, the epiphysis and the hypothalamic region and their relation to diabetes insipidus, Raynaud's disease and other angiospastic conditions, and dystrophia adiposogenitalis. The relation of the epiphysis to macrogenitosomia praecox and the circulation of spinal fluid in the aqueduct is briefly considered. The choroid plexus, the chromaffin system, the thyroid and finally the testicles are considered in turn.

In all of these Löffler pictures the process by which this metabolic correlation is brought about as being dependent on a special arrangement of substances within the cell, changes in surface tension and alterations of osmotic pressures. In this way chemical substances can be separated within the cell itself so that reactions between them become impossible unless the factors enumerated permit it. Sympathicotonia and vagotonia are dismissed by a brief reference to the difficulty encountered in attempting to set up sharply defined clinical pictures.

Altogether, Löffler's article gives a satisfactory, although very brief, review of this intricate subject, without the addition of any original contribution.

WOLTMAN, Rochester, Minn.

PSYCHOLOGICAL TRAITS OF THE SOUTHERN NEGRO WITH OBSERVATIONS AS TO SOME OF HIS PSYCHOSES. W. M. BEVIS, Am. J. Psychiat. **1**:69 (July) 1921.

Bevis presents a psychologic study of the Southern negro. His general conclusions, such as the fact that motion, rhythm, music and excitement make up a large part of the life of the race, that as a people they are carefree and improvident and unable to profit by experience and that fears and superstitions are almost universal among them perhaps merely reflect intellectual immaturity. The negro responds to his environment by a group of relatively simple affective reactions. It has been noted that the formation of a neurosis in the negro usually does not involve a complicated mechanism.

The incidence of cerebrospinal syphilis and paresis was found to be relatively low in the Southern negro. This observation has been made frequently and is by no means peculiar to the negro. Kraepelin originally called attention to the practical immunity not only of the negro, but also of the North American Indian in his natural state, the native Algerian and others. Perhaps this and likewise the surprisingly low percentage of alcoholic psychoses may be interpreted as indicating not only elemental psychologic traits, but anatomically a "lower plane" central nervous system, that is, a lack of complexity and finer differentiation.

Additional statements of interest are that the manic phase of manic-depressive patients is much more usual than the depressed and that dementia praecox is the common psychosis. This is especially true of the catatonic form.

STRECKER, Philadelphia.

NEUROSES IN BUSINESS LIFE. LOUIS CASAMAJOR, Neurol. Bull. **3**: 237, (July) 1921.

Casamajor discusses in a broad way some of the neuroses in industrial life and finds in them the same underlying causes as in the war neuroses. He urges that the idea of traumatic causation be discarded and that study be directed to the personality and psychologic adaptation of the individual. It is his opinion that "workmen's compensation laws . . . for the psychoneuroses have been but stimuli for the development and persistence of sickness"; nevertheless, he feels that desire for compensation alone, which is a conscious process and is malingering, is untrue for most cases. He therefore stresses the unconscious motives in the neuroses of business life as in all other psychoneuroses.

Roughly he groups his cases into: (1) inadequate; (2) dissatisfied, and (3) generally maladjusted. Characteristic of all is the dissatisfaction with the job and the more or less unconscious reason for the conflict.

1. The inadequate person is unequal to his job, fears that he won't succeed and develops his symptoms when responsibility is thrown on him. The result is usually an anxiety neurosis, and his sickness (unconsciously) justifies his failure and saves his self-esteem.

2. The dissatisfied form the largest group. Although equal to the task, these people don't like the work itself, wherefore any trauma is apt to serve as an excuse for giving it up. Most "normals" more or less belong in this group, without, however, crossing the borderline into a true neurosis. Patients of this stamp are easily cured with analysis or suggestion.

3. The generally maladjusted in business life exhibit but a part of their maladjustment to life in general. The latter is really their greater problem. Some of these patients overwork themselves to overcome life's difficulties and often crack as an unconscious way of getting out of them. A slight trauma may be sufficient to cause a litigation neurosis.

WECHSLER, New York.

ON AN ENDOCRANIAL CAST FROM A REPTILE, DESMATOSUCHUS SPURENSIS, FROM THE UPPER TRIASSIC OF WESTERN TEXAS. E. C. CASE, J. Comp. Neurol. **33**:133-147 (June 15) 1921.

The determination of the exact relations between a cast of the cranial cavity and the brain originally occupying that cavity is a matter of great anthropologic significance. In the study of prehistoric remains, whether of

men or other vertebrates, it is necessary to bear in mind that the process of fossilization may yield endocranial casts, endodural casts, or (rarely) actual brain form. These must be carefully distinguished.

In this contribution to paleoneurology the author describes a new endocranial cast of a Triassic reptile and copies from the literature figures of similar casts from several other extinct reptiles. He comments on the fact that in some of these gigantic reptiles the pituitary fossa was very large and says, further: "In all of these forms the brain is exceedingly small relative to the body, but the pituitary body is very large relative to the size of the brain. The connection between the size and activity of the pituitary gland and the size and the proportions of the body in mammals is well known. Hyperpituitarism results in large size or the overdevelopment of certain structures, as the fingers, features, etc. It is equally well known to paleontologists that one of the common variations among those which occur so abundantly in the senile stages of any phylum is giantism. The suggestion naturally arises that the disturbances which arose in the phylum coincident with the development of adverse conditions were primarily of a physiologic character affecting the deep-seated organs of the nervous system and through them, and only secondarily, the superficial structures. The teratologic affections of the pituitary body which produced abnormal structures in a normal phylum may have gradually become a fixed character and resulted in normal giantism in certain groups."

C. J. Herrick, Chicago.

REPAIR OF CRANIAL DEFECTS BY BONE TRANSPLANTS. A COMPARISON WITH CARTILAGINOUS TRANSPLANTS AND ENDOPROSTHESIS. Mauclaire, Paris méd. **2**:153, 1921.

The author reviews the subject beginning with a classification of transplants. His review is an extract of a book, "Etudes sur les Greffes chez l'homme et chez les animaux" (J. B. Ballière et fils), to appear shortly. He divides transplants into two large groups, those of living bone and those of dead bone.

The indication for transplanting is a defect. Contraindications are increased intracranial pressure, underlying chronic meningitis and a defect larger than 6 by 6 cm.

He discusses the varieties of transplants at length and concludes that the results of bone transplantation are good when no cerebral or meningeal complications are present. In such cases the symptoms of the post concussional neurosis remain to some degree. He cites Pièrre Marie's figures on twenty-one cases in which six were improved, twelve remained the same as before filling in of the defect and three were worse. He cites the cases of Henri Claude and Sicard in which the patients were so much worse that the transplant had to be removed.

He ends by saying that the transplants do not remove symptoms of central origin.

Kraus, New York.

ATYPICAL FORMS OF EPIDEMIC ENCEPHALITIS. Charles Rosenheck and Leon H. Cornwall, Neurol. Bull. **3**:259 (July) 1921.

The authors describe six cases with mild signs which they would include in the epidemic forms of encephalitis. One patient gave signs of a mild transverse myelitis, two showed pupillary signs suggestive of syphilis, all had pains which were radicular or neuralgic in character, a few that manifested

distinct depressive and psychoneurotic symptoms, and nearly all had unequal reflexes and hyperesthetic areas. Ordinarily, the authors assert, no diagnosis of encephalitis (or rather meningo-radiculo-myelo-encephalitis) would have been made. A spinal puncture, however, in each case, confirmed their suspicion that they were dealing with atypical forms of the disease. The punctures showed increase of cells, increase of globulin and increased sugar reduction. Negative Wassermann and colloidal gold reactions served to rule out syphilis. They stress the importance of sugar reduction.

It is quite probable that the cases described belong to the epidemic variety, but exception may be taken to the term atypical. Such cases have occurred and the word mild more aptly describes them. Some have undoubtedly been overlooked and more will be missed in the future as the memory of the epidemic slowly fades. The spinal fluid findings do, of course (although not always), help, but one is not always ready to perform puncture on ambulatory patients. It may be said, too, that for every mild, abortive, atypical or ambulatory case that has been overlooked another has been erroneously diagnosed as encephalitis. However, although every neurologist has encountered such cases, the authors have done well to call attention to their occurrence at this time.

WECHSLER, New York.

EPIDEMIC ENCEPHALITIS OF LONG DURATION. S. TURRETTI and G. PIOTROWSKI, Paris méd. **2**:348, 1921.

These authors have found but four cases in the literature of long standing epidemic encephalitis.

The first case recorded by these authors is that of a girl, aged 24, whose illness began in February, 1920, following an attack of "grippe." She suffered from choreiform movements, insomnia, headache and a moderate rise of temperature at the onset. The movements soon ceased. The insomnia at night and somnolence during the day increased. The patient had to be tube-fed. The following September, eight months later, the patient was still very sick, though her condition had improved. She showed marked emaciation. Rigidity of facies and body was still present. Mentally, she was clearer. Her memory for past events was intact.

The second case is that of a man, aged 45, who was taken sick in April, 1917. He presented many of the classical symptoms of epidemic encephalitis. He died in January, 1919, having been sick twenty months. At necropsy, evidence of recent and old encephalitic lesions was found.

The authors believe that these prolonged cases occur in patients showing lethargy, and that the lesion lies in the striate body. They emphasize this by recalling how much the symptomatology resembles that of progressive lenticular degeneration described by S. A. K. Wilson.

This abstract may give the impression that prolonged cases of epidemic encephalitis are rare. This is not the fact, for many can be found in most clinics. The possible duration of the disease still remains to be settled.

KRAUS, New York.

PROGRESSIVE MUSCULAR DYSTROPHY AND THYROID HYPOPLASIA. HELLMUT MAIWEG, Ztschr. f. d. Ges. Neurol. u. Psychiat. **63**: 107, 1921.

In reporting a case of pseudohypertrophic muscular dystrophy complicated by hypothyroidism, Maiweg calls attention to the frequency with which the

former disease is complicated by various other abnormalities, especially of internal secretory functions. Whereas the combination with hypothyroidism has not been described before, the dystrophy has been noted in cases in which there were infantilism, skeletal anomalies, disturbances of intellectual development, exophthalmos, hypertrophy of the tongue, penis, thymus and mammae, hyposexuality and the Chvostek phenomena of tetany. The author lays special emphasis on the disturbance of function of the thyroid and parathyroid glands, and suggests that a more careful examination may reveal hypothyroidism in a greater number of dystrophic cases. In closing he says: "One may look in the future for the consideration of Erb's dystrophy among the disturbances of the internal secretions, especially under hypothyroidism. Its more frequent determination would be of interest not only from the symptomatic and pathogenetic standpoint, but also from the therapeutic standpoint."

FREEMAN, Philadelphia.

MYOCLONIA OR INTENTION SPASM MUCH IMPROVED BY THE ACTION OF LUMINAL. THE ACTION OF LUMINAL IN PARKINSON'S DISEASE. CLOVIS VINCENT and KREBS, L'Encephale **16**:260, 1921; Proceedings of the Society of Neurology of Paris, April 7, 1921.

The patient was a young girl of 19 who had had myoclonia of the left arm for three years. The cause of this myoclonia could not be ascertained. The onset had been sudden. The movements were so severe and fatiguing that the patient had to go to bed at 2 p. m.

These movements were almost completely stopped by 0.15 gm. of luminal taken in the morning by mouth over a period of over thirty days. After the luminal was stopped, the movements did not return at once, but slowly and progressively.

In certain patients with Parkinson's disease, neither able to walk nor write, luminal relieved the spasm of the extremities sufficiently to permit both walking and writing. In certain cases, scopolamin in small doses was given with the luminal with better results. Great care, however, must be taken in the administration of this drug.

All cases of Parkinson's disease are not favorably influenced by luminal.

KRAUS, New York.

TRAUMATIC LESIONS OF THE PITUITARY BODY, AND MULTIPLE CRANIAL NERVE PARALYSES. L. REVERCHON, G. WORMS and ROUQUIER, Presse méd. **29**:741 (Sept. 17) 1921.

This detailed and interesting case history, with two radiograms and three other illustrations, includes the findings at necropsy after several months' observation. Numerous authorities are quoted and compared in reference to each feature of the case.

A line of fracture passed through the posterior part of the sella turcica. The fifth, sixth and seventh nerves were bilaterally affected, and there was general clinical evidence of a hypopituitary condition—especially polydipsia, polyuria, severe anemia, asthenia and arterial hypotension with mental infantilism and lethargy. At necropsy the hypophysis was found reduced to a mere fibrous shred without a trace of normal tissue to be seen under the microscope.

Another traumatic case, reported previously by the same authors, is contrasted with this; the other patient developed tumor of the hypophysis and a different clinical picture.

HUDDLESON, New York.

OCULAR MANIFESTATIONS IN ENCEPHALITIS LETHARGICA. GUSTAVUS I. HOGUE, Am. J. Ophth. **4-8**:592 (Aug. 21) 1921.

The ocular symptoms are frequently the first to be complained of; the order of frequency is diplopia, ptosis, impaired accommodation, optic neuritis and papilledema.

The diplopia is due to motor paresis or paralysis, is of central origin and usually transient. The nerves involved in order of frequency are the sixth, third and rarely the fourth. Ptosis is usually bilateral, usually partial and due to weakness of the levators. Ptosis combined with ocular paresis is pathognomonic of epidemic encephalitis.

Impaired accommodation is manifested by quick fatigue. This symptom is the last to leave; the pupils are sluggish, irregular, unequal bilaterally, and nystagmus may be slight or pronounced. There is no lagophthalmus.

Optic neuritis is manifested by blurring of the disk and is probably due to dilation of capillaries with resultant congestion of the retinal veins.

Papilledema is a rare condition—there are only two cases on record—and when present is due to subcortical hemorrhage or edema cerebri or both. Four cases are reported.

DELONG, Philadelphia.

EPIDEMIC ENCEPHALITIS AND PREGNANCY. TRANSMISSION OF THE DISEASE BY THE MOTHER TO THE FETUS. RICARDO JORGE, Paris méd. **2**:454, 1921.

The reported results gathered by Jorge of epidemic encephalitis in pregnant women are grouped as follows:

1. The encephalitis was cured, and the pregnancy was normal.
2. The encephalitis ended in death before delivery.
3. The encephalitis ended in death after spontaneous delivery.
4. Delivery was artificial in order to save the patient.

Of the twenty-five cases reported, the fetus was still-born in three. In sixteen cases no record was made, owing to the death of the mother before the end of pregnancy. In five cases (20 per cent.) the fetus lived. Two of these showed typical myoclonic movements. In one, these stopped in five days. No notes of the others are present. One patient died a few days after birth, and at necropsy showed signs of encephalitis.

The author concludes that congenital epidemic encephalitis exists.

KRAUS, New York.

DIPLOMYELIA. F. KINO, Ztschr. f. d. Ges. Neurol. u. Psychiat. **65**:272, 1921.

Kino's patient was a man, aged 36, who died eighteen months after an apoplectic stroke. The spinal cord was found duplicated from the third to the fifth lumbar segments. Diagrams of the cord at different levels are given by the author, showing how the doubling of the cord resulted from incomplete fusion of the two sides above and below, with the development of two morpho-

logically well formed spinal cords between the levels named. The author distinguishes several differences, however. The most important is a complete absence of any pyramidal degeneration in the accessory cord. In addition, whereas the cells of the dorsal horns are well formed, those of the ventral horns are few in number and poorly developed.

The author suggests that embryologically there was an imperfect fusion of the two sides of the neural fold.

He comments on the relative longevity of his patient and of the other subjects reported in the literature.

FREEMAN, Philadelphia.

NERVE TERMINATIONS IN THE LUNG OF THE RABBIT. O. LARSELL, J. Comp. Neurol. **33**:105-131 (June 15) 1921.

Our knowledge of both the anatomy and the physiology of the intrinsic innervation of the lung is very meager. In this study, sensory and motor nerve endings of the rabbit were investigated by the methylene blue method. Three types of sensory endings were found in the epithelium of the primary bronchi and at the points of division of the succeeding orders of bronchi. The most distal endings were observed in the walls of the atria. These are adapted to respond to stretching of the walls of the atria during inspiration, and may represent the receptors responsible for the inhibition of inspiratory movement and the excitation of expiration. The nerve endings in the larger bronchi are regarded as tactile in function, responding to the presence of foreign bodies; those in the smaller passages may be stimulated by pressure during expiration. Motor endings were found both in the smooth muscle of the bronchial tree and in the muscular coat of the pulmonary vessels.

C. J. HERRICK, Chicago.

THE PLATELET COUNT AND BLEEDING TIME IN CATATONIC DEMENTIA PRAECOX. SCHICHI UYEMATSU, Am. J. Psychiat. **1**:15 (July) 1921.

Uyematsu finds that the bleeding time is decidedly lessened in catatonic dementia praecox, the average being 3.3 minutes, while the blood platelet count is markedly increased. The same relationship between the coagulation period and the number of platelets has been described in hypothyroid states, notably myxedema and cretinism. The author's study was stimulated by the clinical similarity existing between clearly recognized and relatively well understood minus thyroid conditions and dementia praecox, which is still a closed book. The attempt to read its pages by the light of endocrinology is fascinating, but still barren of result. Uyematsu deserves credit for his careful investigation and the presentation of a not unimportant point. However, even his rather conservative conclusion in favor of an endocrine origin for catatonic dementia praecox is somewhat in excess of the premises.

STRECKER, Philadelphia.

EFFECTS OF CUTTING THE LINGUAL NERVE OF THE DOG. J. M. D. OLMSTED, J. Comp. Neurol. **33**:149-154 (June 15) 1921.

Taste buds are known to disappear from the tongue after severance of their nerve supply, but there are differences of opinion as to the manner of

this disappearance, whether by degenerative destruction of their cells or by dedifferentiation and return to the form of indifferent epithelial cells. On experimentally cutting one lingual nerve of the dog, the taste buds supplied by this nerve disappear within fifteen days. The process is a true degeneration with the aid of phagocytic leukocytes, not a dedifferentiation or metamorphosis of the cells.

C. J. Herrick, Chicago.

DIAGNOSIS OF THE LOCATION AND NATURE OF A CERTAIN VARIETY OF CEREBRAL TUMORS (PSAMMOMA OR ANGIOLITHIC SARCOMA) BY ROENTGEN RAY. Souques, L'Encephale **16**:259, 1921; Society of Neurology of Paris, April 7, 1921.

Roentgen-ray examination showed with great clearness and precision not only the place, but also the form, volume and relations of the tumor in question. The patient presented signs of tumor. The tumor was removed and the patient was cured. Histologic examination showed that the tumor was a psammoma. The author emphasizes the importance of the roentgen ray in detecting this kind of tumor since it is benign and easily removed.

Kraus, New York.

Society Transactions

NEW YORK NEUROLOGICAL SOCIETY

Three Hundred and Ninetieth Regular Meeting, Oct. 4, 1921
FOSTER KENNEDY, M.D., *President, in the Chair*

A CASE OF ACUTE INFECTION OF THE SPINAL ROOTS AND PERIPHERAL NERVES (NEURONITIS). DR. THOMAS K. DAVIS.

Dr. Davis presented the patient, a man 20 years of age, who had been a marine fireman. His family history was negative. He had been a hard worker and had enjoyed good health. He had used neither alcohol nor drugs and had had no venereal infection. A year ago, in the South, he had had an illness, probably typhoid, for two months. Several months after recovery, he noticed a weakness in the arms, which was not very disabling. This continued for two months when weakness began in the left leg, progressed and became quite severe. Soon it extended to the right leg, so that in the third month of these symptoms he was unable to walk except with assistance. Bladder control was good; there were no eye symptoms and no somnolence; no signs of epidemic encephalitis.

When first examined, one month ago, he had nystagmus to the left and sometimes to the right. Otherwise the eyes were normal, as were swallowing and speech. There was weakness of the flexors of each arm and of the left triceps. The muscles of the trunk were normal. There was weakness above and below the knees and complete foot drop. Both knee jerks and ankle jerks, and in the left arm biceps and triceps jerks, were absent. The right abdominal reflexes were diminished, the left absent.

He had a left tactile hemianesthesia, not total; on the right he had a stocking type of anesthesia. The hemianesthesia stopped very distinctly at the midline and above at a line with the angle of the jaw. Sensation in the fingers and toes was normal. There was some atrophy of the flexors of the arms, considerable in the legs, but no tenderness on pressure along the nerves. The electrical responses showed very slight variations from the normal.

His general health was good, and we found no evidence of disease of the internal organs. The spinal fluid Wassermann reaction was negative. The spinal fluid had 11 cells per c.c.; the blood pressure was 110; the blood count normal.

He received massage and galvanism and has shown improvement. The abdominal reflexes are now stronger. The knee jerk can be elicited on the right with reenforcement, and there is a partial return of sensation in the right foot.

We believe the symptoms to be due to some toxic insult to the peripheral nerves, probably with root involvement. That the sensory symptoms and the chief motor symptoms are both on the same side, that the sensory symptoms on the right side are of the stocking type, that the spinal fluid is normal, that the condition of the patient is improving, and chiefly the fact that there are electrical changes of a quantitative type, all urge this diagnosis, to the exclusion of another, which perhaps the single finding of nystagmus would lead us to,

namely, multiple sclerosis. We feel that the case has a good prognosis with the former diagnosis. The coordination is very good. During the early stage he had slight pain in the legs, but it was very indefinite, and for the most part he has not had true neuritic pain.

CASE OF GASOLINE OR GAS POISONING. Dr. W. M. Kraus.

The patient had been working about a gas engine, exposed to the fumes from the exhaust. He started at that particular job in August, 1920, and entered Bellevue Hospital in June, 1921. At that time he was troubled with numbness, weakness and coldness in the legs, hands and forearms. This trouble had begun the previous February, when he complained of dizziness, some nausea, frontal headache and general malaise. He had had no stomach attacks before that. His legs slowly became weaker, so that by April he could not walk. He had no other symptoms.

Examination revealed that his cranial nerves were normal. He had a bilateral hypesthesia of a stocking variety almost to the knees, and in the toes a slight defect in sense of position. He was unable to stand. Six blood Wassermann tests and three spinal fluid Wassermann tests were negative. The last blood Wassermann test was followed in about three weeks by administration of arsphenamin.

At present he shows marked atrophy of all extremities. He has no more fibrillary twitching. His superficial reflexes are still present. He shows no signs of involvement of any of the other nerves.

Thompson in his book on "Occupational Diseases" states under the heading "Gasoline Poisoning":

> Lately there have been brought to my notice a number of cases of chauffeurs who were overcome by gasoline fumes, while cranking motor cars in small, unventilated garages. They experienced vertigo, fainting and nausea, and one passed into coma from which it was difficult to arouse him. The symptoms may not develop until the men go into the open air.

About three years ago three men in one garage complained of the same symptoms, and they told me at the time that this disease was not infrequent among chauffeurs. Since then I have had another case, but it has not shown the profound atrophy found in this case.

DISCUSSION

Dr. Kennedy: Perhaps Dr. Kraus did not see a paper by Gowers about fourteen years ago. Gowers had a group of gasoline poisoning cases very much resembling vasoneurosis, but without any involvement of the nervous system as such.

Dr. E. G. Zabriskie: My impression of this case is that the poison came from the exhaust, as in most other reported cases. Therefore, it is carbon monoxid poisoning, and it is incorrect to speak of it as gasoline poisoning.

EVIDENCE OF NERVOUS CONTROL OF SOME INTERNAL SECRETIONS. Dr. Walter B. Cannon.

Dr. Cannon of the Medical School of Harvard University (by invitation) presented this paper illustrated with lantern slides.

He said: My interest in the control of internal secretion began years ago in the observation that emotional excitement inhibits the movements of the stomach and intestines. This inhibition is due to sympathetic impulses. The

question arose as to whether the suprarenal medulla, which is also innervated by the sympathetic, and whose output, epinephrin, mimics sympathetic action in the body, is likewise caused to secrete during emotional stress.

Evidence of Nervous Control of the Suprarenal Medulla.—The first evidence was given by de la Paz and myself in 1911. An intestinal strip rhythmically contracting in oxygenated Ringer's solution is promptly inhibited by very minute amounts of epinephrin. It is likewise inhibited by blood removed from the inferior cava in the neighborhood of the lumbosuprarenal veins, if the blood is taken during or immediately after excitement, but is not inhibited by blood taken from the same region before excitement. The conclusion was, therefore, that the suprarenal medulla discharges into the blood stream its peculiar product, epinephrin, in periods of emotional stress. Later Hoskins and I showed that asphyxia and afferent stimulation, both of which are known to cause discharge of sympathetic impulses, would likewise induce extra suprarenal secretion. The evidence thus adduced has been confirmed by various observers using various methods.

The results and the methods leading to the conclusions just mentioned have been criticized by Stewart and Rogoff. They have declared that the output of epinephrin from the suprarenal glands is not increased by asphyxia, by reflex stimulation, or by excitement; indeed, that the secretion is constant and unchangeable. Furthermore, they urge that it is impossible to judge whether the secretion is increased or decreased unless the rate of blood flow is known. I shall now defend the method that we employed, though it can be defended by use of recent evidence that the blood flow in the inferior cava is, in fact, increased under the conditions cited, so that the tendency would be not to concentrate a constantly secreted epinephrin but rather to dilute it; in other words, the conditions of blood flow are unfavorable to the result which de la Paz, Hoskins and I actually found. Instead of elaborating this line of defense, I wish rather to speak of the use of the denervated heart as an indicator of suprarenal secretion. In this preparation both vagus nerves are severed, the stellate ganglions are removed, and the heart itself, separated from the central nervous system, remains in the body performing its function, but no longer subject to any influences except those brought by the blood stream. The heart thus isolated is extraordinarily sensitive to circulating epinephrin. Though very stable in its rate, it can be made to beat faster by 30, 40 or 50 beats per minute, by inducing asphyxia or reflex stimulation or excitement. Such increases, in our experiments, did not occur or were relatively slight when the suprarenal glands were removed. The same conclusion was drawn, therefore, that was drawn from the earlier experiments, that the three conditions just named induce a secretion of the suprarenal medulla.

Again, the evidence has been criticized by Stewart and Rogoff. They repeated the experiments with the denervated heart and found that in certain instances they were able to obtain a marked increase of rate though the suprarenal glands had previously been removed. Examination of their results showed that the order of increase of the two conditions was as a rule very different. The heart rate is on the average increased approximately 30 beats per minute if the suprarenal glands are intact, whereas the increase is only 6 beats per minute after their removal. Nevertheless, there are instances of much larger increases than the average of 6 beats, and these cases must be accounted for.

In experiments in cooperation with Rapport, I have found that the increase cannot be explained by increased blood pressure, arterial or venous, by warmer blood from the abdomen, or by accessory suprarenal tissue. It dis-

appears, however, on severance of the hepatic nerves. For the present we may leave the matter at this point and understand that after cutting these nerves reflex stimulation or stimulation of an afferent nerve will produce a pure suprarenal effect.

In explanation of the faster heart rate when the suprarenal glands are present and an afferent nerve is stimulated, Stewart and Rogoff have suggested that such stimulation by constricting the splanchnic area induces a "redistribution of blood in the body," with consequent concentration of the uniformly secreted epinephrin which thereby becomes more effective. This suggestion we have put to test. By use of a "reduced" animal (that is, one with both carotid, both brachial and both renal arteries tied, the aorta closed below the renals and the mesenteric nerves cut) vascular reflexes are reduced to a minimum. The liver is excluded and the suprarenals are the only abdominal viscera left innervated. Typical reflex accelerations of the denervated heart averaging 26 beats per minute occurred in this preparation, although the changes of blood pressure were insignificant or were only moderate variations either up or down. Under these circumstances the more rapid beat cannot be attributed to a redistribution of blood in the body, for the possibilities of redistribution were lacking.

Another test consisted in substituting a stream of epinephrin to replace suprarenal secretion after removal of the glands. In this test epinephrin at the rate of approximately 0.0007 mg. per kilo per minute were injected by vein continuously to replace the continuous unvarying suprarenal secretion assumed by Stewart and Rogoff to be normal on the basis of a test made inside the body. Typical vascular reflexes were now induced by afferent stimulation, but either no acceleration of the pulse occurred or an insignificant increase of two beats per minute. Again the evidence is clear that the faster rate is not due to redistribution of blood in the body.

Stewart and Rogoff have urged that their method is superior to all others in being quantitative. We have found that the denervated heart in the "reduced" animal can be employed as a means of assaying epinephrin. Repeated equal doses injected intravenously at a uniform rate repeatedly produced in a given animal the same or nearly the same maximum increases of heart beat, and if the injections differed in rate the increases likewise differed in degree. When the circulation is thus simplified the acceleration is wholly or chiefly dependent on the rate of injection, and is little, if at all, influenced by variations in blood pressure. By matching epinephrin injections with reflex epinephrin secretion we have shown that when the heart rate is accelerated between 30 and 42 beats per minute, the output from the suprarenal glands lies between 0.0032 and 0.0037 mg. of epinephrin per kilo per minute, i. e., more than ten times the amount regarded by Stewart and Rogoff as the unvarying normal secretion, when they removed blood and assayed it for epinephrin outside the body. Stewart and Rogoff have not explained the discrepancy between their figures for the internal and the external test for suprarenal secretion.

From the foregoing evidence we conclude that the results of our earlier work have been verified and that the inferences drawn from it that suprarenal secretion is evoked by asphyxia, by reflex stimulation and by excitement, are justified.

Evidence for Nervous Control of the Liver.—It will be recalled that the denervated heart may be made to beat faster on afferent stimulation though the suprarenal glands have been removed, and that this effect is abolished by

severance of the nerves to the liver. The inference was drawn, therefore, that some agent arising in the liver is the occasion for the faster rate. Uridil, Griffith and I have shown that stimulation of the hepatic nerves themselves will cause an increased rate of the denervated heart, an effect appearing slighty later than the similar epinephrin effect and lasting for a longer time. Furthermore, stimulation of the hepatic nerves will cause a rise of blood pressure. This is not due to a retarded blood flow through the liver, for it does not occur on closure of the hepatic artery and vein. Furthermore, it does occur on hepatic stimulation though all the abdominal viscera have been removed except the liver, and unlike the stimulation of splanchnic blood vessels alone it long outlasts the period of stimulation.

The increments of heart rate vary widely. They are slight if the animal has been fasting or is in poor condition. They are much greater if the animal is digesting meat. These conditions can account for the discrepancies between Stewart and Rogoff's results and those reported by myself and my collaborators as a consequence of splanchnic or reflex stimulation after removal of the suprarenal glands. The inference is drawn, therefore, that a substance is given off by the liver into the blood stream which, carried to the denervated heart, raises its rate.

None of the known or supposed products of hepatic activity—glucose, urea or catalase—when injected into the blood stream have the effects produced by exciting the hepatic nerves. Furthermore, the efficacy of hepatic stimulation in causing a faster heart rate when meat is being digested is not seen in an animal which is digesting carbohydrate or fat or which has been fed for several days on either of these foodstuffs. On the other hand, stimulation is more effective after amino acids have been injected into the intestines.

For the present we are inclined to draw the conclusion that probably the effects noted are not due to a true internal secretion produced by the liver, but to a discharge from its cells of amino acids or amins which are sympathomimetic in character.

Evidence of Nervous Control of the Thyroid.—Histologists have described nonmedullated nerve fibers reaching to the cells of the thyroid gland. Anatomists find that fibers going to thyroid gland arise in cervical sympathetic ganglions. It had been shown that severance of its cervical sympathetic nerves would cause atrophy of the thyroid and that stimulation of these nerves would cause a diminished iodin content of the gland. In 1916, in cooperation with Cattell, I pointed out that if the thyroid gland and neighboring indifferent tissue are connected through a galvanometer, stimulation of the cervical sympathetic cord evokes an action current after a latent period varying between 5 and 7 seconds, an effect which persists after the superior and the recurrent laryngeal nerves are severed. This does not occur if the main trunk of the vagus is stimulated, or if pilocarpin, which is an excitor of vagus endings, is injected. The effect cannot be attributed to the shutting off of the blood supply by vasoconstriction, for total anemia produced by clamping the carotids for a period equal to that of sympathetic stimulation produces no noteworthy electrical changes in the gland. From this evidence the conclusion was drawn that the nerve fibers distributed to the thyroid cells belong to the sympathetic and not to the cranial division of the autonomic system, and since their effects are not indirect through alterations of blood supply, they are indeed true secretory nerves.

The electrical evidence given in the foregoing was confirmed by the experiments of R. L. Levy in the Harvard Physiological Laboratory in 1916. He

made use of the fact that if thyroid substance is introduced into the blood stream standard doses of epinephrin injected intravenously will cause a greater rise of blood pressure than they caused previous to the thyroid dosage. He found that after stimulation of the cervical sympathetic there was indeed a greater effectiveness of the suprarenal injections amounting in some instances to as much as 200 to 300 per cent.

Since epinephrin will mimic the action of sympathetic impulses it was to be expected that epinephrin itself would stimulate the thyroid. This Levy found to be the case. Stimulating doses of epinephrin caused a greater effectiveness of the repeated standard doses (which were very small). If the thyroid glands had previously been removed, however, both cervical sympathetic stimulation and epinephrin injections failed to produce an increase of the response to the standard doses.

Levy proved that stimulation of the cervical sympathetic caused the thyroid to secrete promptly. Removal of the thyroid gland immediately after stimulation was followed by an increase of the efficacy of the standard doses similar to that observed when the glands were left in the animal throughout the course of the experiment.

The method used by Levy involved destruction of the upper part of the central nervous system. This procedure did not permit tests to be made on the effects of such stimulation as would in the conscious animal be accompanied by sensations of pain, nor on the influence of asphyxia. It was desirable to have another method, therefore, which would permit such tests as these to be made.

In 1920, in cooperation with P. W. Smith, I made use of the denervated heart as an indicator of thyroid activity. If the temperature of the animal is kept uniform and the ether anesthesia is likewise maintained at a fairly uniform level, the heart rate does not vary more than a few beats a minute through several hours of observation.

With the denervated heart we found that gentle massage of the thyroid gland for two or three minutes would cause an increased rate amounting in some instances to 33 per cent. over the basal rate. The development of the maximal increase was usually slow, requiring from 30 to 60 minutes and passing off in a similarly slow manner. Massage of another gland, for example, the submaxillary, did not cause this effect, and massage of the thyroid would cause it even though the suprarenal glands had previously been removed.

We found that stimulation of the cervical sympathetic trunk would induce a similar increase of the rate of the denervated heart, but that this did not occur if the thyroid gland had previously been removed on the stimulated side. Reflex stimulation of the sciatic or brachial nerve, such as would cause sympathetic impulses to be discharged, had the same effect as direct stimulation of the cervical trunk, but with the addition that there was a primary acceleration of the heart due to suprarenal discharge. This was followed by the same slowly developing thyroid effect. Asphyxia, likewise, had the same influence. On the other hand, if the thyroid glands had been previously removed sensory stimulation and asphyxia induced only the increased rate due to suprarenal discharge.

The foregoing evidence—electrical, cardiac and vascular—is harmonious in testifying to a control of the thyroid through the sympathetic strands in the neck. By connecting the phrenic nerve with the cervical sympathetic and

waiting until the phrenic fibers have grown out to the distribution of the sympathetic fibers we have been able in a certain percentage of cases to produce many of the phenomena of hyperthyroidism. When this operation is done, there would be discharged to the distribution of the cervical sympathetic nervous impulses with every respiration. Thus, nerve filaments affecting the thyroid would be continuously excited by normal impulses from the central nervous system. In animals which showed indications of hyperthyroidism there was an increased heart rate running up from the normal, which in the cat is approximately 150 beats per minute, to 225 and even 250 beats per minute. The character of the animal changed from that of a quiet affectionate domestic pet to that of a furtive creature running away from any one entering the room. There were loose movements of the bowels without evidence of fermentative changes. The skin of the neck and head became thickened in spots, and there were patches of alopecia. The most characteristic alteration, however, was a great increase in metabolism. In one of the animals the increase of the metabolic rate was 125 per cent. Removal of the thyroid gland on the operated side restored the animal in which the metabolism had been greatly increased to a rate which was within the normal range of variation.

The Nature of the Nervous Control.—Thus far evidence has been adduced that suprarenal secretion, an output of a stimulating material from the liver, and likewise thyroid secretion, are all subject to sympathetic impulses, that is, to the activity of that part of the nervous system which is roused in great excitement. It is important to know where the reflex center for this activity is and whether the control of the organs mentioned is double as in the case of the submaxillary gland (chorda and sympathetic), or single like the biceps, for example, with inhibitory or excitatory impulses affecting the central station.

Answers to these questions have been found for the suprarenal medulla in experiments performed by Rapport and myself. We have observed that reflex acceleration of the denervated heart can be evoked after removal of the cerebrum and the corpora quadrigemina. A transection a few millimeters back of these bodies destroys the reflex.

Removal of the suprarenal glands (without a fall of blood pressure) lessens the rate of the denervated heart. Under experimental conditions, therefore, a continuous suprarenal discharge is going on. Using this as a basis we have regarded slowing of the rate of the denervated heart in consequence of stimulation as being due to diminished suprarenal secretion. Such slowing occurs when the central end of the vagus or of the depressor nerve is stimulated in the cat. It may be as much as 24 beats per minute and appears even after the corpora quadrigemina and all parts anterior to them have been removed. It fails to appear when the brain stem is severed a few millimeters back of the corpora and likewise after the suprarenal glands have been exercised. We conclude, therefore, that the reflex center for suprarenal secretion is located near the upper or front edge of the floor of the fourth ventricle and that it is subject to both excitatory and inhibitory nervous influences.

The evidence just adduced places the control of suprarenal secretion in a part of the primitive nervous system. It is interesting to note that this region is in close approximation to that which appears to be concerned in emotional expression. Sherrington and Woodworth were able to call forth pseudo-affective reflexes in the cat after ablation of the hemispheres and the thalamencephalon, and Goltz likewise noted many evidences of emotional expression (snarling, snapping, etc.) in a dog from which the cerebral cortex had been

removed. Head and Holmes, because of clinical studies, were led to the conclusion that the thalamus in man is concerned with the affective side of sensation. It appears, therefore, that the emotional reactions of animals are not managed in the neopallium but are ingrained in an archaic portion of the nervous system. Possibly it is for this reason that the expression of emotions is so similar in widely different animals, such as the cat or the dog, and man. The fact, however, that such is the case permits us to use the lower animals in an analysis of conditions accompanying emotion outbreaks and justifies the expectation of finding similar conditions and a similar analysis in human beings. The great interest of study along lines here hinted at lies in the association of primitive and archaic reactions occurring in an ancient portion of the nervous organization of the body and operating on an elementary form of the nervous system (the sympathetic) with the functions of organs which are fundamentally important for the organism—the ductless glands.

DISCUSSION

PROF. FRANK H. PIKE of Columbia (by invitation): It is a peculiar pleasure to me to discuss this paper, for two reasons: For many years Dr. Cannon and I have been friends; that is the first reason. In the second place, it is not very often that physiologists get a chance to talk to neurologists.

It has long been recognized that the production of an electrical current is an invariable accompaniment of the processes of living matter. Walter has gone so far as to characterize the production of electrical current as a sign of life.

Whenever a gland secretes, whenever a muscle contracts, there is an electrical current produced; and today we make wide clinical application of the fact that the heart produces an electrical current; that this current can be led off and its variations measured with precision. We judge the minute changes in the heart merely by its electric cardiogram.

There can be no question, I think, that a change in the electric condition of a gland or muscle indicates some change in its degree of metabolism. One may object that the changes are small; that there may be other tissues in that gland aside from gland cells, and it is not always easy to separate the gland cells from the other tissues. The objection probably has some weight; but I think, taking into consideration the facts that go along with it, that there is some nervous control of such an organ as the thyroid.

With regard to the suprarenals, I am frankly puzzled. I did not come here with the intention of saying anything about our own results. It seems strange that Dr. Cannon, beginning with a study of the intestine, should find that the root leads back to the nervous system. It seems perhaps strange that for myself, beginning with the study of the central nervous system, I should somehow or another get into this suprarenal mixup. Yet, such is the case.

I do not care to say much about interpretations now; I am frankly puzzled. But, if you will pardon me, I will depart from my original intention and briefly describe some things which bear on this question.

Some years ago Professor Stewart and I (this makes matters worse) tied off the blood vessels to the head of a cat. We obtained a very vigorous response. We obtained a great variety of blood pressure, we obtained changes of heart rate, we obtained changes of respiration, and the whole picture was rather characteristic. While about two or three years ago, possibly four, Mrs. Lincoln set about analyzing the vasomotor pathway more in detail—we

wanted to find out where these vasomotor fibers came out; we wanted to find out, first, whether it was the vasomotors that were concerned. We believe now that those vasomotor fibers are concerned. But rather a peculiar thing was noticed. This tying off of the blood vessels to the brain, of course results in total failure to function. After a time the animal lies as if dead. If you keep up artificial respiration, the heart and the entire other part of the system is open, so that the head is the only part deprived of oxygenated blood. It does not do the cat much harm to kill it; in fact, we kill the cat several times. We find that this procedure of tying off the blood vessels for three or four or five minutes, whatever is necessary (allowing this whole change of blood pressure, heart rate, whatever else it is that happens, to go on, until there is a failure of the function of the medulla), restoring of circulation, getting the cat back again, can be repeated as often as eighteen times. The cat seems to be very resistant. Yet, to all appearances, this is a pretty severe procedure.

It was noticed, however, that as the cat was subjected to these successive insults, that the blood pressure curve began to show some differences. We had at first simply the curve rising, a little dip in it, and another rise, and another fall. It was plainly enough one curve. But after these blood vessels had been tied off, say, ten or twelve times, or eight or nine times, depending on the cat, we began to find a notch in the top of that curve; there was a partial drop in that blood pressure, then it came back again, and we obtained a final fall. Toward the end of this series we found that the blood pressure rose pretty sharply and fell perhaps way down to its normal level and again rose. We obtained this curve breaking up in two; it was pretty symmetrically divided in point of time, but a repetition of this process for three hours did not suffice to kill the cat.

The chance observation that the blood did not clot very readily in such a cat made us suspect that the suprarenals might be involved here, so we tied off the suprarenals. There is no particular change in blood pressure when this is done, but the picture subsequently is rather different. In some cats, immediately after typing off the suprarenals, simply tying off the blood vessels once is sufficient to curtail this response by about 50 per cent. We get simply the sharp rise and the blood pressure falls, say, within half the time occupied by the control. Sometimes four or five conclusions are necessary, but invariably the animal dies; we find that it is impossible to bring it back, after this procedure has been employed a few times. There is something in that epinephrin which is extremely necessary for this relationship between smooth muscle and sympathetic nerve. I have an idea that without the suprarenals, neither people nor animals would be able to stand nearly as much as they do in this world. It might be better for some of us if we could.

It seemed to us rather important that we could shorten this survival time of an animal after the removal of its suprarenals, say, to a period of fifteen minutes and not have to wait twelve or twenty-four hours for it to die. The longest time any of them have survived is a day and a half. There are so many of them, I am not perfectly sure of the figures, but the time was of that order of magnitude.

That is about as far as we have progressed, and, thinking the matter over, I can see that there is enough to keep me busy probably for the next generation, working out some of these matters in detail, but I suspect that somewhere in our work there are conditions which we have not yet taken into account, which are causing these discrepancies. Where they are, I do

not know, but I do not feel like talking about them now. The safest way by far, I think, is to go back to the laboratory and interview the cat about it. The response, I think, would be far more trustworthy than anything I might say.

Again I must express my pleasure at the presentation Dr. Cannon has given us, and if we disagree in matters which seem to you trivial, mere squabbles among ourselves, I hope you will forgive us. The thing promises some interesting discussion somewhere, until we run down these conditions which lie at the bottom of our discrepancies.

Dr. Walter Timme: The general proposition, I venture to think, is that the physiologists, while they are rather accurate in the work they do, nevertheless do not agree among themselves as to observations or conclusions even from the same observation. I do not think that we are very much worse off than they. We travel, perhaps, a little faster; I do not know that we get much further. And so the subject is left more or less as Dr. Pike left it, and that is as far as we get.

To me it seems that Dr. Cannon's work is not destructive but constructive. Stewart and Rogoff seem to be quite destructive. So that for the present, and I hope for the future, I and all of us will be able to bear with Dr. Cannon in his further work.

I should like to ask Dr. Cannon if stimulation of the sympathetic nervous system or a portion of it, or of a peripheral nerve, will produce an increase in the suprarenalin content of the blood. Then, if it is granted that epinephrin in the blood stimulated the sympathetic nervous system (and I think that has been proved), we are immediately confronted by a condition of unstable equilibrium in which epinephrin stimulates the sympathetic, the sympathetic stimulates the epinephrin production, and so we go on with a constantly increasing production of epinephrin until finally, if the production went on ad infinitum, exhaustion would ensue. There must therefore be an inhibitory center which automatically must bring that process to cessation, or else we would all succumb; as a matter of fact, we do in certain instances succumb to the stimulation of the suprarenal medulla by means of thyroid activity. The administration of thyroid in a susceptible person will produce syncope, due, in all probability, to exhaustion of the suprarenals through that administration.

Dr. Cannon has explained to us that the center for these reactions is probably in the thalamic region. That perhaps is the old brain center of affective motor control. There must be a higher center in the brain which, through its stimulation and its emotions, controls this lower center, and, although Monakoff has not definitely pointed out such pathways, I have wondered whether since his time physiologists such as Cannon have had any experience in their laboratory with the physiology of those pathways.

Dr. Pike made the statement, which had been made previously a number of times, that the activities of all organs, and especially of the internal secretory organs, could be enhanced or produced not only by one method but by many. That leads me to this statement: A great deal of the outpouring of epinephrin and of thyroid secretion is perhaps not due to nerve current at all but to direct massage, just as he explained, experimentally, it could be produced. Deep breathing, ordinary respiration, with the movement of the diaphragm downward, compressing the abdominal organs, in itself has the mechanical and physical effect of producing and of releasing into the circulation a certain amount of epinephrin so that massage (physical causes, as well as electrical ones) will suffice to produce the secretion.

Then there is a third one, a direct chemical effect, as has been shown a number of times, I believe, especially in the frog's heart removed from the body. A frog's heart removed from the body will beat rhythmically for a certain number of hours if properly maintained in the proper fluids; but if a little thyroid solution be added to that, the number of hours will be enormously increased. The direct chemical effect, therefore, besides the physical, besides the electrical, is an element of the production of the internal secretory flow into the circulation.

Dr. Israel Strauss: Dr. Timme has stated that in the cases of certain persons if you administer large doses of thyroid, you may get syncope, and that that is due to extra excretion of epinephrin, due to exhaustion of epinephrin. I want to ask Dr. Cannon if there is any physiologic proof of that. Dr. Cannon has shown on his charts that when epinephrin is introduced, the activity, through thyroid, is increased. I believe that is what the chart shows. But I believe he has not shown us whether increased administration of the thyroid has any effect on the suprarenal.

It is just such physiological facts that are necessary to us clinicians.

I am very anxious to know whether Dr. Cannon has proved, or if he can quote work which will show definitely, that the thyroid secretion given to an animal can influence the suprarenal glands as he has shown that the suprarenal gland can affect the thyroid gland.

Dr. Stewart Paton of Princeton (by invitation): I think probably Dr. Cannon does not need to be reminded that we have a very excellent opportunity of studying these archaic reactions in the embryo of all the higher vertebrates. I feel that my opportunity has come, because I have been repeating for a number of years that somebody with Dr. Cannon's capacity for conducting experiments should try to correlate the functions of the embryo in relation to the development of the nervous system.

As I have said before, first the thyroid comes into the circuit and then the suprarenal; and I was very much interested to hear Dr. Cannon speak about the abundant supply of the suprarenal. That is my feeling, and still I would like to keep the question open.

I was also glad to hear him refer to the center for the nerves that supply these organs, the corpora quadrigemina in that region. From the long fasciculus which develops in the embryo you can trace the nerve supply down to the thyroid and you can trace that fasciculus, getting down very much later to the suprarenals, at least comparatively speaking; so that the thyroid comes into the circuit quite a good deal ahead of the suprarenals, and it seems to me that if we observe the embryo very carefully at this time, a good deal of information can be obtained in regard to the functions of the thyroid and the suprarenals in relation to the nervous system.

I was glad to hear Dr. Pike say a word about the nervous system, because I know a physiologist whose attitude toward the nervous system is very similar to that of an English clergyman I met, who asked me what I was working at. I said, "The brains of a fish." He said, "Do fish have brains?" I said, "Yes." He said, "By Jove, that's quite an idea!"

Dr. Cannon: Dr. Pike's description of the discharge from the suprarenal gland, the evidence at least in the experiments that he and Stewart did together, that there is a storage in the suprarenal gland when the blood supply is cut off from the medulla, seems to me to fit in with all the evidence I have presented in regard to the suprarenal, in regard to the nerve supply, causing

a rise in blood pressure that is well known and developing cerebral compression, for example, and would therefore cause a discharge of impulses similar to that which was illustrated this evening, and a discharge which would result in an increased output of epinephrin from the medulla and a consequent rise of blood pressure—that rise with a dip that was pointed out two years ago in an observation that was made in the Harvard Laboratory, and was also noted by Elliott, and later worked out in more detail by Vincent. That disappeared, you will recall, in Dr. Pike's experiments, after the suprarenal glands were tied off. In those experiments, after the suprarenal glands were tied off, the animals, he said, died sooner than was the case when the glands were not tied off. Now he drew the conclusion that that indicated the importance of the suprarenal medulla. I do not believe that he is really justified in drawing that conclusion because when he ties off the suprarenal glands, he ties off two glands of the suprarenal medulla and cortex.

Then Dr. Timme raised a very large question as to whether the physiologists had any advantages over the clinicians as workers in the realm of science. I think I am not perhaps misstating his remarks; at least, he suggested that inasmuch as there are differences of opinion among clinicians and also differences among physiologists, that they are both on the same plane in that they have differences; but it seems to me that the physiologists at least have this advantage over the clinicians: they work on material and concern themselves with problems which are capable of demonstration and capable of being inquired into under conditions which are more or less controllable.

For instance, take the question that Dr. Strauss referred to in Dr. Timme's remarks, the question whether syncope is due to exhaustion of the suprarenal because of feeding the thyroid. Now, it seems to me that there are a number of wholly unwarranted assumptions in that statement. In the first place, it is possible to remove both suprarenal glands from animals, and they will live for hours. We have had them live for three or four days, with no drop of blood pressure whatever. It is a very common assumption, that the suprarenal medulla is necessary for the maintenance of blood pressure. Now, that is not at all a demonstrated fact. In all probability the fall of blood pressure that occurs finally after removal of both suprarenal glands is a consequence, possibly, of the removal of the suprarenal cortex rather than the medulla; a general weakening of the whole animal. At least, it is possible to remove practically all of the medulla from the suprarenal glands, leaving the cortex, and the animals will survive for a long period; and then on removing whatever is left, they promptly succumb. I do not know of any evidence that the feeding of the thyroid produces any exhaustion whatever of the suprarenal medulla; in other words, the feeding results in an increase in the size of the suprarenal glands, and, as I showed you this evening, that involves both the cortex and the medulla. So that we are at least capable of trying out these matters and getting at the evidence under conditions which are, I think, more largely under control than is the case in practice on the human being.

So far as the difference between Dr. Stewart and Dr. Rogoff and myself is concerned, that is a matter of getting out the evidence. After this evidence was obtained, I was kindly invited to go to Cleveland and speak to the Cleveland Academy of Medicine, which I did in May, and presented this material, and Dr. Stewart listened to it and did not offer any constructive criticism, and I do not see that he has very much to do except to find out now what is wrong about his own experiment.

As I told you earlier, most of this material is not yet published. The work on the suprarenal gland will come out in the December number of the *American Journal of Physiology*, and I hope that when that evidence is before investigators it will be convincing. I am ready to demonstrate any point that I have made here this evening, at any time, to any group of physiologists or other interested people, so that if there is any question in any of your minds as to whether this evidence is on the same basis with clinical evidence, you can take at least that method of discriminating between two types of evidence.

There is another point that Dr. Timme brought up, and that is why we did not get into a regular cyclone of sympathetic activity because the sympathetic stimulates the suprarenal, the suprarenal stimulates the sympathetic, and so the circle goes, until we wind up in a catastrophe. Well, that might occur if the suprarenal gland were subject to suprarenal secretion, but there is no evidence that it is, and there is quite a bit of evidence that it is not. It has a peculiar nerve supply; it is not enervated by the sympathetic as the blood glands are enervated by the sympathetic. So that the possibility of a vicious circle being started is not present, and, as the record showed this evening, after an afferent nerve is stimulated or the splanchnic nerve is stimulated, the heart rate goes up promptly, beginning about eleven or twelve seconds, and in perhaps a minute and a half after the stimulation has ceased, the heart rate is down where it was before. In other words, this vicious circle does not occur, and it does not occur because the secreted epinephrin does not in turn stimulate the secreted cells to produce more epinephrin.

With reference to a higher center for emotion than those which are found in the thalamic region, I should like to have more evidence. It seems to me that the observations of Head and Holmes were extremely interesting observations, and if you will recall that the study of their cases indicated that these persons had an exaggerated affective accompaniment of all sensation, just in so far as the evidence indicated that this thalamic center was disconnected from control by the cortex, and they suggested that here was a mechanism in the thalamic region already for expression of affective states of emotional experience and that it was inhibited by the cortex, and that when the cortical inhibition was removed, this lower center then had full freedom of expression. The experiments of Sheridan and Woodworth and also the observation of Goltz indicated likewise that this lower mechanism was quite adequate at least for the expressions of intense emotion such as rage, or possibly fear; certainly the aspect of attack is shown by these animals quite apart from any play of the cortex, because the cortex has been wholly removed.

Book Reviews

PSYCHOLOGIE DER WELTANSCHAUUNGEN (PSYCHOLOGY OF WORLD VIEWS). By KARL JASPERS. Pp. 428. Berlin: Julius Springer, 1919.

The work claims to be an attempt, "only to understand what final positions the soul assumes and what forces move it." The author realizes the weighty objections that may be made to his venture in this field, but justifies his efforts by the conviction that his presentation in orderly form of whatever has already been recognized as psychology of "Weltanschauungen" would in itself offer something of value. Hegel, in the "Phenomenology of the Spirit," is the author's only great predecessor, but Jaspers differs in not erecting a uniform, systematic structure but in presenting a schematic arrangement, not following one method like Hegel, but now this, now that one. Kant, Nietzsche and Max Weber offer much in support of the view of the author, as does also the Dane, Kierkegaard, whom he regards with great favor.

Jaspers would divide the whole psychology of "Weltanschauungen" into three parts: (1) general—basic principles, limits, forces, etc.; (2) the spheres of personality, of activities like science, metaphysics, art, religion, etc., and the social sphere, applying to a greater or less extent the categories of Part 1; (3) the most concrete and actual part, consisting, because of the mass of material, in studies of single personalities, peoples, ages, conditions, or professions, etc.

Jasper's present work is limited to the first of these parts: general principles presented in an attempt at order—at completeness of principles, if not of material. It consists of three long chapters with subdivisions. Chapter 1 is on the various standpoints (Einstellungen, "attitudes," "adjustments," "focusings"): objective (active, contemplative, mystic—all with subclasses); enthusiastic (covering self-sacrifice, love, creative impulse, etc., with a few words on ecstatic conditions and intoxication). Chapter 2 treats varieties of the "Weltbild," which is, as it were, the shell in which we live and which limits our world for us—"we do not see what lies beyond for we do not even suspect its existence." Here again there is a threefold division: the material-spatial, the psycho-cultural and the metaphysical worlds. Within the last group Jaspers presents material from Goethe, illustrating that author's conception of the "demonic" element in his world; in another section there is a discussion of the types of philosophical thinking (illustrated by the Greeks). The last chapter is on "types of mind," under which term "principles, ideas, forces, are understood"; the effort is made to bring these directly into the field of vision and, by reversing the process of the first two chapters, to trace their manifestations in "Einstellungen" and "Weltbilder." Tables of values are suggested and discussed (the highest good, ultimate aims, etc.); the antinomic structure of existence, optimism and pessimism, skepticism, etc., nihilism in psychoses (pp. 265 to 269) Jasper illustrates by a case which he reported in 1913 in the *Zeitschrift für die gesamte Neurologie und Psychiatrie.* In appropriate places in all parts of the work the author inserts in fine print

summaries of the pertinent thought of various philosophers; in the last chapter ten such pages are devoted to Kierkegaard. There is a twenty-page appendix on Kant's doctrine of the idea.

A full table of contents only partly makes up for the lack of an index.

Jaspers does not pretend to give a direct answer to the concrete questions of actual life; his book makes an appeal to the "free intellectuality and activity of life by offering means of orientation, but it does not attempt to create and teach life" (preface). The volume represents a most serious effort to handle a large subject. It is possible that in a later edition the author will find occasion to rewrite, revise and clarify many sections of the work. It is not a book which a beginner in philosophical study would pick up, or profit much from if he did. Only those with much experience in philosophical reading in German are advised to peruse the work—it is not merely the occasional pleasant compound like "das Sichdurchsichtigwerdenwollen" (p. 94) that he will have to swallow.

THE NERVOUS CHILD. By HECTOR CHARLES CAMERON, Physician to Guy's Hospital. Pp. 202. London: Oxford University Press; American Branch, 35 West Thirty-Second Street, New York, 1919.

This little book so handy in size and so well printed, as is apt to be the case with English books, on lightweight paper, contains a modicum of common sense. The author defines his work as an introduction to the teachings of psychology, physiology, heredity and hygiene as applied to medical work in nurseries and in schools. But it goes beyond any introductory standpoint; it is in places, especially in therapeutic recommendations, baldly concrete and specific.

Nervousness, to Dr. Cameron, seems to mean anything and everything. As a matter of fact, "The Nervous Child" is for him a thread on which he strings many discrete ideas—imitativeness, the child's love of power, the reasoning of childhood, sex teaching, etc. As a matter of fact, actual nervous symptoms and tendencies, such as habit spasm, hysterical phenomena, sleeplessness, receive the most scant treatment, even by way of an introductory statement.

There is a deal that might be said concerning Dr. Cameron's therapy: Hypnotism is one of his main standbys. We read that in inveterate cases of enuresis hypnotic suggestion is always, "I think," successful. Concerning chorea: "Hypnotism is often almost instantly successful, but, apart from hypnosis, curative suggestions proceeding from the attendants form the principal means at our disposal." And there are many other citations which might be made that seem curious to the reviewer.

But there is a background of extremely good sense which the author has developed from his acute observations. It is evidenced by his insistence on the value of justice and truthfulness to the child, by his remarks on the value of close understanding of children, and the winning of their confidence, and the insight that is necessary for the proper understanding of childhood life. That it represents a fair introduction to the best in either psychology or medicine we seriously doubt. It leaves the reviewer still looking forward to the publication of a really good book on the functional nervous disorders of childhood.

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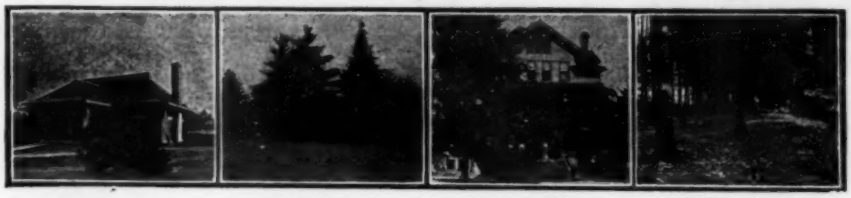

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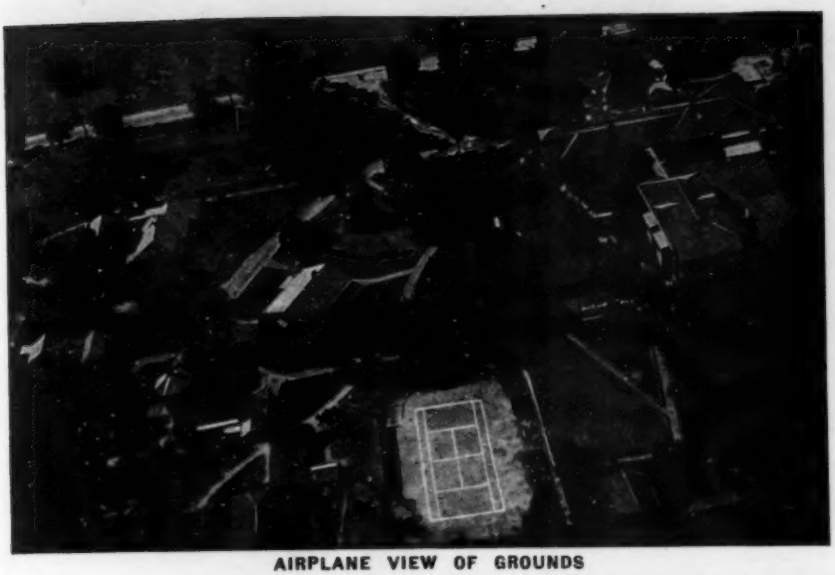

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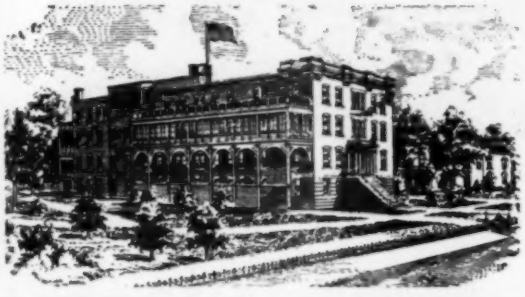

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